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THE

POETRY OF ASTRONOMY

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THE POETRY OF ASTRONOMY:

A SERIES OF FAMILIAR ESSAYS ON THE HEAVENLY BODIES, REGARDED LESS IN THEIR STRICTLY SCIENTIFIC ASPECT THAN AS SUGGESTING THOUGHTS RESPECTING INFINITIES OF TIME AND SPACE, OF VARIETY, OF VITALITY, AND OF DEVELOPMENT.

BY RICHARD A. PROCTOR,

AUTHOR OF "SCIENCE BYWAYS," "THE BORDERLAND OF SCIENCE," ETC.

Profounder, profounder man's spirit must dive :
To its aye-rolling orbit no goal can arrive :
The heavens that now draw him with sweetness untold
Once found—for new heavens man spurneth the old.—EMERSON.

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MANY readers of KNOWLEDGE, *in which Weekly Paper an effort has been made to bring scientific news and discoveries before the public in a cheap and trustworthy form, have asked me whether I could not publish cheaper editions of some of my own books. In the great majority of cases, the matter does not rest in my own hands; but having had an opportunity to purchase from Messrs. Smith, Elder, & Co., their rights in my three works,* "THE BORDERLAND OF SCIENCE," "SCIENCE BYWAYS," *and the* "POETRY OF ASTRONOMY," *I have much pleasure in so far acceding to the above-named wishes, as to publish an edition of each of these works at two-thirds of their original price. Owing to the circumstance which led to this, I have thought it desirable to class those three books as the first three volumes of a series to be called the* KNOWLEDGE LIBRARY. *Most of the volumes of the series are to be republications of Essays by various Contributors to the Magazine from which the series takes its name.*

RICHD. A. PROCTOR.

LONDON, *September*, 1882.

PREFACE.

Many think that science cannot truly be called science if clothed in poetic garb, and, on the other hand, others seem to fear that a glory must depart from the face of nature if science scrutinise her mysteries too closely. I believe both these fears to be unfounded—that science need not be less exact though poetry underlie its teachings; while, beautiful and glorious though the ordinary aspect of nature may be, a deeper poetry, a more solemn significance, a greater beauty, and a nobler glory can be recognised in the aspect of nature when science lifts the veil which hides it from the unaided vision. Nay, I believe that no one who studies aright the teachings of the profoundest students of nature will fail to perceive that our Galileos, Keplers, and Newtons, our Priestleys, Faradays, and Tyndalls, have been moved in no small degree by poetic instincts, and that their best scientific work has owed as much to their imagination as to their reasoning and perceptive faculties. And, on the other hand, we find abundant evidence in the works especially of modern poets that the truths of science are even more impressive than the more direct and obvious teachings of nature. I have had these considerations in view in preparing the essays in the present volume. On the one hand, I have not been deterred by scientific scruples from presenting the poetic aspect of recent astronomical discoveries, nor, on the other, have I feared lest the recognition of the real significance of these discoveries should do aught but enhance our conceptions of the glory and splendour of the universe.

RICHARD A. PROCTOR.

NEWCASTLE, NEW SOUTH WALES:
August 30, 1880.

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THE POETRY OF ASTRONOMY.

AGE OF THE SUN AND EARTH.

Raphael. The sun, as in the ancient days,
'Mong sister stars in rival song,
His destined path observes, obeys,
And still in thunder rolls along.

Gabriel. The vex'd sea foams—waves weep and moan,
And chide the rocks with insult hoarse,
And wave and rock are hurried on,
And suns and stars in endless course.—GOETHE.

WE have learned how small is our domain in space, but as yet we have scarcely been willing to admit that man's duration in time is as utterly minute, and in a sense insignificant. Yet there is scarcely a feature of our recently acquired knowledge about the relations of the earth in space which has not its parallel in known facts respecting time and the earth's relations thereto; while the mysteries of space, as yet unfathomed and unfathomable, have their analogues in the mysteries which a thoughtful mind recognises in relation to time, as well in the remote past as in futurity. We may hereafter consider specially in these pages the parallelisms of time and space. At present we note only that the subject we have to deal with illustrates strikingly the manner in which the researches of modern science into space relations lead men to consider also the periods of time during which the objects of their research must have existed in the past and are likely to exist in the future.

In the infancy of human thought it was a sufficient explanation of the light and heat of the sun to suppose that a bright and hot body circled around the earth (or rather round the place inhabited by the observer), coming into view each day in the east, and passing over by the south towards the west. Rejoicing as a giant to run his course, never varying in his circuit round the earth, the sun was regarded either as himself a being of power, or else as representing the energy of a higher power, which had set this glowing mass in the sky, and had appointed its courses. But while on the one hand the sun was regarded as a smaller body than the earth, so unquestionably the duration of the sun was regarded as of necessity less than that of the earth. For ages this earth had endured, without form and void, cold and dark, before the sun was appointed to gladden her with his beams; and though the future was not so clear to men's minds, yet it was generally supposed that the end of the earth would not come while the sun and the moon endured.

The recognition of the vast superiority of the sun over the earth in size was not attained gradually, as some have asserted, but suddenly. The discovery came on men as a revelation. One generation had believed in a central earth, all-important in the universe, as well in space as in time. In the lifetime of the next generation the earth had descended from her high position to become one only and by no means the chief of several small bodies circling round the giant orb of the sun. No longer central in space, she could no longer be regarded reasonably as central in time; in other words, it was no longer reasonable to suppose that her formation, however brought about, her progress, however long-lasting, and her final end, however attained, either marked the beginning, progress, and end of time, or occupied a central position in all time. We do not find that men were as ready to accept this conclusion as they had been (no choice, indeed, being left them) in accepting the earth's non-central position in space. But the inference was undoubtedly the only reasonable and probable one. The earth's history *might* no doubt occupy a central position in time, precisely as this day on which we write these lines may be exactly midway between the day when life first began on the earth and the day when life here will finally cease. Yet, while either proposition *might* be true, one is not more wildly improbable than the other.

With regard to the sun, which had now come to be recognised as exceeding the earth more than a millionfold in size, it was an equally reasonable inference that his duration also far surpassed that of the earth. Of course the substance of either might reasonably be regarded as existent during all time; but the fashioning of the mighty orb ruling over a family of which the earth was but a small member, might reasonably be supposed to have belonged to a far more remote epoch than that of the earth; and his continuance as a sun might as reasonably be supposed likely to outlast, not merely by many centuries, but many *times*, the continuance of our earth as the abode of living creatures.

Men had no positive evidence, however, on these points, so long as they considered only the dimensions of the sun and earth. It was natural to suppose—or rather it would have been natural, for as a matter of fact the supposition was not entertained—that as the duration of mankind far surpasses the duration of a nation, and as the duration of a nation far surpasses that of any individual man, so the duration of the solar system, and therefore of the ruler of the system, must far surpass that of any individual planet. But there was only one way (one general way involving many special methods) of determining whether this was actually the case or not; and the researches of men along this special line of research did not begin till long after the importance of the sun in size had been ascertained. We refer to the inquiry into the processes actually taking place in the earth, in the sun, and in the solar system, and into the evidence respecting the continuance and effects of such processes in the past. Men's ideas on some of these points were almost as vague at the beginning of the present century (nay, even much later) as had been the thoughts of the men of old times respecting the proportions of the heavenly bodies and their orbits. We find Sir W. Herschel, for instance, adopting and enforcing a theory respecting the sun's condition, and the emission of solar light and heat, which would not account for one week's supply of such sunlight as we actually receive. Still later we find a man like Dr. Whewell, a skilful mathematician and an able physicist, who also, if not strictly speaking an astronomer, was well read in astronomy, maintaining in his 'Plurality of Worlds' the theory that the fixed stars may be mere lights, not mighty masses like our sun—a theory which the modern discovery of the conservation of force shows to be utterly inconsistent with the steady emission of enormous quantities of intensely brilliant light during many thousands of years.

But now the student of science recognises in the sun's constant radiation of light and heat the existence of a store of energy which must have been in some way garnered up during long past ages. As certainly as the constant deflection of the earth from the direction in which she is moving at any moment indicates the existence of a force residing in the sun towards which body that deflection constantly takes place, so certainly does the emission of light and heat from the sun indicate the action of processes in the past by which the necessary energy has been stored up. We know that the sun cannot be the habitable orb girt round by phosphorescent cloud-masses imagined by Sir W. Herschel, any more than it can resemble the stars, as imagined by Dr. Whewell, in being a mere light without any considerable mass or substance. The working of a steam-engine does not more certainly indicate the consumption of fuel, and therefore the prior gathering together of fuel, than does the sun's radiation of light and heat imply the consumption of solar energy, and therefore the prior gathering together of stores of energy.

When this was first recognised, students of solar physics were content to inquire how the observed emission of solar light and heat could be accounted for in such a way as to explain the sun's appreciably unvarying size and mass. They perceived that to regard the sun as a mere mass of burning fuel would by no means suffice. We can measure the quantity of heat that the sun constantly emits, because we can measure the amount received by our earth, which intercepts about one-2,300,000,000th part of all the light and heat emitted by the sun. We thus find that in every second of time the sun emits as much heat as would result from the combustion of 11,600 billions of tons of coal. In passing, it may be convenient to notice that each portion of the sun's surface as large as our earth emits as much heat per second as would result from the combustion of a billion tons of coal—a simple and easily remembered relation. Now it is easily calculated from this that if the sun's whole mass consisted of coa and could burn right out to the last ton, maintaining till then the present rate of emission, the supply would not last more than 5,000 years. As the sun has most certainly been emitting light and heat for a far longer period than this, the idea that the solar fire is thus maintained is of course altogether untenable. There are, however, many other reasons for rejecting the idea that the sun is composed of burning matter, using the word 'burning' in its proper sense, according to which a piece of coal in a fire is burning, whereas a piece of red-hot iron is not burning, though burning hot. In like manner we find ourselves compelled to reject the belief that the sun may be a body, raised at some remote epoch to an intense heat throughout its entire mass, and gradually cooling. For we find that in the course of a few thousands of years such a mass would cool far more than the sun has cooled (if he has cooled appreciably at all), even within the historic period; and we have evidence that he has poured his heat on the earth during periods compared with which the duration of the human race is but as a second amid centuries, while the duration of historic races is utterly lost by comparison.

This brings us to the consideration of evidence which has only in quite recent times been brought to bear on the question of the sun's age.

We know from records left by men of old times that the sun was in their time very much what he is now, though we cannot be altogether certain that he gave out exactly the same amount of light and heat, or even almost exactly the same. Again, from the remains of animals and plants in the earth's crust we can deduce similar inferences. Those animals and plants could not have existed unless the sun had supplied light and heat as at present, though we cannot assert so confidently that he supplied the same amount of either. The possible range of variation may have been greater, so far as evidence of this kind is concerned, than in the case where we have human records for our guidance. But there is other evidence which, while less exact still as to the actual emission of light and heat, ranges over periods of time far greater than could be directly inferred from the examination of fossil fauna or flora. As yet we are not able to form satisfactory estimates of the periods of time necessary to bring about such and such changes in the various races of plants and animals; hence, although we may be quite sure that enormous time-intervals must have elapsed before the races whose remains only are found became changed into the races which are their modern representatives, we cannot definitely assign the duration of these time-intervals, or even at present make the roughest approximation to their length. But there are changes depending on the sun's action whose rate of progress we can satisfactorily measure. We know that processes of change are caused on the earth's surface by the downfall of rain and snow, by the action of frost and ice, of winds and waves, by chemical action, by processes of vegetation, and other causes, all depending on solar activity. Geologists no longer assign the existing irregularities of the earth's crust to causes other than those at present at work, or even suppose that, within the range of time over which their researches extend, causes such as these acted much more actively than they do at present. But it may be noted in passing, that, so far as those causes of change are concerned which depend on solar action, it will not greatly affect the argument now to be brought before the reader, whether we consider the activity of such causes to have been widely variable in the past, or to have been appreciably uniform. For it will be seen that the chief difficulty we shall have to encounter resides in the necessity of explaining the total amount of solar radiation in the past. If, in order to shorten the time-intervals indicated by those features of the earth's crust which depend on causes of change due to solar action, we imagine those causes to have once operated far more actively than at present, we necessarily assume that the sun's action was far more intense then than now. We manifestly gain nothing, so far as this special difficulty is concerned, if we have to enhance our conceptions of the solar radiation in the same degree that we reduce our estimate of the time during which his rays have been at work upon the earth.

But in reality we are not free to imagine any very noteworthy change in the conditions under which the earth's surface has undergone change during the greater part of the time over which geological researches extend. For there is evidence proving that the progress of changes in the past must have resembled that taking place at the present time. Consider, for instance, the evidence afforded by the various strata which have been deposited at the bottom of the sea. In these strata are the remains of creatures which formerly existed in the sea; and we find these remains in such a condition in many instances as to prove, beyond all possibility of doubt or question, that, unless those creatures were *much* more short-lived than their present representatives, the average rate of deposition must have closely resembled that now recognised in similar seas. As Lyell remarks: 'When we see thousands of full-grown shells dispersed everywhere throughout a long series of strata, we cannot doubt that time was required for the multiplication of successive generations; and the evidence of slow accumulation is rendered more striking from the proofs, so often discovered, of fossil bodies having lain for a time on the floor of ocean after death before they were imbedded in sediment. Nothing, for example, is more common than to see fossil oysters in clay, with serpulæ, or barnacles (acorn-shells), or corals, and other creatures, attached to the inside of the valves, so that the mollusk was certainly not buried in mud at the moment it died. There must have been an interval during which it was still surrounded with clear water, when the creatures whose remains now adhere to it grew to a mature state.' Nay, there are cases where we have evidence of still slower deposition than could be thus inferred. For we often find that the creature which has attached itself to the shells of defunct mollusks have not only grown to maturity before the shells were covered with deposited matter, but have in their turn died and their hard coverings have been slowly pierced by other creatures, while still the deposit had not covered the shell of the mollusk to half its thickness.

It may appear at first sight that evidence about the rate of deposition of mud at the bottom of the sea does not bear very obviously on the question of the sun's radiation of light and heat. But it must be remembered that this deposition of matter measures the rate at which matter has been carried away from the earth's surface above the sea-level; while the rate at which this process—or what is called 'sub-aërial denudation'—takes place, depends on the downfall of rain and snow, the action of wind and storms, and other causes depending on the energy of the solar rays.

We may turn, then, with sufficient confidence to the evidence which the earth's crust affords respecting the time during which the solar radiation has continued. We certainly are not likely to obtain an estimate in excess of the truth, apart altogether from the consideration that there may have been, and most probably were, enormous periods of time during which the sun's rays were poured on the earth without producing any effects which can now be recognised, and most probably still more enormous periods before the earth had a crust at all, when the solar radiation was already intensely active.

The evidence derived from the earth's crust, however, will be found sufficiently striking, without our entering into the consideration of possibilities relating to preceding eras. 'When we reflect,' says Mr. James Croll, whose researches into this and related subjects are full of interest, 'that with such extreme slowness do these agents' (rain, snow, ice, running water, &c.) 'perform their work, that we might, if we could, watch their operations from year to year, and from century to century, without being able to perceive that they make any sensible impression, we are necessitated to conclude that geological periods must be enormous.'

Let us follow Mr. Croll in his consideration of a few of the many facts bearing on this point. (Much that immediately follows here is simply translated into popular language from a very interesting article by Mr. Croll in the 'Quarterly Journal of Science' for July 1877.)

It is well known that many parts of the earth's surface which now show no marked inequalities were formerly the scene of great dislocations (not necessarily produced suddenly), when the surface on one side of the line of dislocation had been depressed hundreds or even thousands of feet below the surface on the other side of that line. On the present surface no signs of these tremendous displacements (whether produced by upheaval or by sinking, or by both) can now in general be recognised, the inequalities having been all removed by denudation. But to effect this levelling, a mass of rock must have been removed equal in thickness to the extent of the dislocation. If we can ascertain the full depth of the stratum thus removed, and also the average rate at which denudation takes place, we shall have a measure of the length of time required for the levelling process. Only, at the outset, we must remember, first, that an estimate thus formed is likely to fall far short of the truth, even as respects the particular process involved; and, secondly, that that process is in itself but one in a series of such processes. We learn from a fault, as a dislocation of this kind is called, how much more has been denuded on one side than on the other to restore the level; but not how much has also been taken from both sides. Again, where a fault of this kind occurs, the strata which have undergone the process of dislocation are commonly themselves the products of denudation from other surfaces existing, of course, long before the dislocation occurred. And these surfaces in their turn were probably the results of slow processes of deposition of matter denuded from still earlier surfaces.

To consider, however, a few examples of extensive faults.

Professor Ramsay, describing some of the remarkable faults in North Wales, states that near Snowdon there is a fault where the displacement of the strata amounts to 5,000 feet, and in the Berwyn Hills one of 5,000 feet; in the Aran range occurs the Bala fault, with a downthrow of 7,000 feet. Between Aran Mowddwy and Careg Aderyn the displacement is between 10,000 and 11,000 feet. 'Here we have evidence,' says Mr. Croll, 'that a mass of rock, varying from one mile to two miles in vertical thickness, must have been denuded in many places from the surface of the country in North Wales.'

'Along the flank of the Grampians a great fault runs from the North Sea at Stonehaven to the estuary of the Clyde, throwing the old red sandstone on end sometimes for a distance of two miles from the line of dislocation.' Professor Geikie concludes that the amount of displacement must be in some places not less than 5,000 feet.

But perhaps the most remarkable instance known is that of the great fault which crosses Scotland from near Dunbar to the Ayrshire coast. On the south side of this fault we find the ancient silurian rocks, north of it the less ancient rocks, the old red sandstone and carboniferous of North Scotland.[1] The amount of dislocation is in some places fully 15,000 feet, or nearly three miles. Now, it is to be observed that the dislocation is older than the carboniferous era. For originally the silurian rocks south of the fault must have been covered by the prolongation of the old red sandstone, afterwards completely removed by denudation. If the carboniferous strata had then existed, they, lying uppermost, would, of course, have been washed away first. But we find them on the south side of the fault, lying immediately on the old silurian floor, the old red sandstone which originally covered that floor having been entirely removed. Thus the 'enormous thickness of nearly three miles of old red sandstone must have been denuded away during the period which intervened between' its deposition and the subsequent accumulation of the carboniferous lime-

[1] It is absolutely necessary here, and in what follows, to use these technical geological terms. For the subject of our present inquiry it will suffice to say that the carboniferous rocks are later than the old red sandstone (at least in any given geological district), the old red sandstone later than the silurian, while the Laurentian rocks, mentioned further on, are older yet than the silurian. Of course the oldest rocks lie lowest.

stone and the coal measures now lying directly on the silurian rocks!

One other case to indicate the enormous periods required for the formation of some of the features of Scottish scenery.

Professor Geikie has shown that 'the Pentlands must at one time have been covered with upwards of a mile in thickness of carboniferous rocks, which have all been removed by denudation.' 'Now,' says Mr. Croll, 'the Pentlands themselves, it can be proved, existed as hills in much their present form before the carboniferous rocks were laid down over them; and as they are of lower old red sandstone age, and have been formed by denudation, they must consequently have been carved out of the solid rock between the period of the old red sandstone and the beginning of the carboniferous age.'

But, in order fully to appreciate the vastness of the periods required for these and kindred changes, it is necessary to recognise the extreme slowness with which such changes proceed.

The first calculations directed to the solution of this difficult problem were those made by Manfredi in 1736. In 1802 Playfair took up the inquiry. But the materials available at that time were so imperfect that these earlier calculations were not satisfactory. In 1850 Tylor, from a careful investigation of the evidence respecting the quantity of matter brought down by rivers into the sea, deduced the conclusion that 10,000 years would be required to raise the sea level by three inches. More recently Mr. Croll, from the latest measurement of the sediment transported by European and American rivers, calculated the rate at which the surface of the land is being denuded. 'The conclusion arrived at in his able memoir,' says Sir Charles Lyell, 'was that the whole terrestrial surface is denuded at the rate of one foot in 6,000 years; and this opinion was simultaneously enforced by his fellow-labourer, Mr. Geikie.' This was in 1868.

It may be well, before considering the bearing of these researches on the subject presently before us—the obliteration of the effects of dislocations in the earth's crust—to quote the opinion of Sir Charles Lyell on this method of dealing with the general problem of terrestrial denudation. 'It is evident,' he says, 'that when we know the dimensions of the area which is drained, and the annual quantity of earthy matter taken from it and borne into the sea, we can affirm how much on an average has been removed from the general surface in one year; and there seems no danger of our overrating the mean rate of waste by selecting' (as Mr. Croll and Geikie had done) 'the Mississippi as our example. For that river drains a country equal to more than half the continent of Europe, extends through twenty degrees of latitude, and therefore through regions enjoying a great variety of climate; and some of its tributaries descend from mountains of great height. The Mississippi is also more likely to afford us a fair test of ordinary denudation, because, unlike the St. Lawrence and its tributaries, there are no great lakes in which the fluviatile sediment is thrown down and arrested on its way to the sea. In striking a general average we have to remember that there are large deserts in which there is scarcely any rainfall, and tracts which are as rainless as parts of Peru; and these must not be neglected as counterbalancing others in the tropics where the quantity of rain is in excess. If then, argues Mr. Geikie, we assume that the Mississippi is lowering the surface of the great basin which it drains at the rate of one foot in 6,000 years, 10 feet in 60,000 years, 100 feet in 600,000 years, and 1,000 feet in 6,000,000 years, it would not require more than about 4,500,000 years to wear away the whole of the North American continent if its mean height is correctly estimated by Humboldt at 748 feet; and if the mean height of all the land now above the sea throughout the globe is 1,000 feet, as some geographers believe, it would only require 6,000,000 years to subject a mass of rock equal in volume to the whole of the land to the action of sub-aërial denudation. It may be objected that the annual waste is partial, and not equally derived from the general level of the country, inasmuch as plains, watersheds, and level ground at all heights remain comparatively unaltered; but this, as Mr. Geikie has well pointed out, does not affect our estimate of the sum total of denudation. The amount remains the same; and if we allow too little for the loss from the surface of table-lands, we only increase the proportion of the loss sustained by the sides and bottoms of the valleys, and *vice versâ.*'

We may note, in passing, that, adopting the estimated rate of denudation here indicated, the actual time required for the entire submergence of the present continents, if no vulcanian forces were at work to prevent submergence, would not necessarily be even approximately represented by the period of 6,000,000 years mentioned above. At the outset the rate of submergence would be greater than the mere rate of denudation, since every foot removed from the surface of the continents would cause a rise of about $4\frac{2}{3}$ inches in the level of the sea; so that at first the surface of continents would be lowered on the average not one foot only in 6,000 years, *with respect to the sea-level*, but 1 foot $4\frac{2}{3}$ inches. On the other hand, as the continents became greatly reduced in extent, it is probable that the average annual rate of denudation would be diminished, the portions still remaining above the sea-level being of harder and more durable material than those which had been removed. We need not inquire further, however, into the question here raised, which, though suggested by our subject, does not, strictly speaking, belong to it; moreover, in nature the process considered cannot take place, the earth's internal forces constantly restoring the balance between land and water by the upheaval of submerged regions.

For the purpose of our present inquiry the action of the earth's vulcanian energies need not be considered; because we are concerned only with the question how long would be the period of time required for the removal of a stratum so many hundreds or thousands of feet in thickness. We know, certainly, that, in the special cases we have to deal with, strata of such and such thickness were removed; and it matters little whether, as the process of removal went on, they were being steadily raised by the earth's subterranean action, or whether the original dislocation was followed by the sudden raising of the strata at one side of the fault and their equally sudden lowering on the other side. However the difference was brought about, it is certain that the raised strata were worn down eventually by the steady action of the same causes which wear down the general surface of the large continents. Having ascertained the average rate at which these causes work, we can apply the result to determine how long they would be in producing the observed levelling down of the upraised strata in faults. There is no reason for supposing that in the remote past the process would go on more quickly than at the present time. And we have seen that even if it did, that would imply a greater activity in the solar energies to which these processes are all in reality due, so that our difficulty would be in no way diminished by any such assumption. The time required would be reduced by a few millions of years perhaps; but the difficulty we are dealing with is not a question of time at all. We are inquiring now into the amount of the total expenditure of solar energies in past ages; and the time-intervals indicated by the earth's crust are only of importance in so far as they show how vast that expenditure of energy has been. Doubtless, in considering other questions, the length of these time-intervals is a question of great interest, but it does not directly concern us here.

Let us, however, follow Mr. Croll in recognising the possibility that, in some of the cases we have to deal with, the rate of denudation may have been greater than the average rate inferred from the consideration of river drainage. To prevent the possibility of over-estimating the periods necessary to effect the observed denudation, let us assume the rate to have been double the average rate, or equal to one foot in 3,000 years.

At this rate a thickness of three miles which (*at the very least*) has been swept away in South Scotland since the old red sandstone period would require 45 million years!

But, older than the old red sandstone rocks, the silurian formations have been denuded in places to depths of thousands of feet before the old red sandstone strata were deposited. And these ancient formations were themselves deposited in the ocean by the slow denudation of the Cambrian rocks. These in turn had been formed from the earlier Laurentian strata. And lastly (so far as the researches of geologists at present extend), the Laurentian rocks themselves were built up from the ruins of other rocks which were themselves sedimentary rocks, not the actual primary rocks of our globe. We should almost certainly underestimate the period required for these processes of denudation preceding the old red sandstone era, if we assumed that it was only equal in length to the period which has elapsed since that era. But making this assumption, and assuming also (which is also almost certainly an under-estimate) that the interval which has elapsed since the old red sandstone era is 45 million years, we find a total of 90 million years for the stratified rocks. In other words, we find at least 90 million years for the period during which rain has fallen on the earth as at present. During that time, therefore, the sun has poured his rays upon the ocean, raising up their waters by evaporation to be carried by winds (also generated by the sun) over the continents, and there discharged in the form of rain and snow.

It may be noticed, in passing, that Sir William Thomson infers, from the observed underground temperature of our earth, that the consolidation of the crust cannot have taken place less than 20 million years ago, or the underground temperature would have been greater than it is; nor more than 400 million years ago, or the underground temperature would have been less than it is. The limits are rather wide; but a value well within these limits would accord with Mr. Croll's estimate of 90 million years as the interval since the earliest stratified rocks were deposited.

Now, the difficulty thus raised is this:—At present we know of no way in which the sun could have emitted the same amount of heat as at present for anything like this period of 90 million years, without having shrunk to much smaller dimensions than he at present has.

It is generally admitted by physicists and astronomers that the solar heat has had its origin in the main, almost wholly in fact, in processes of contraction; and that it is maintained by such processes. In other words, the gravitation of the sun's mass has given birth to all, or very nearly all, the heat which the sun has emitted in the past, and will continue to emit till the end of his career as a sun. It was once supposed that meteoric downfall on the sun's surface produced the chief share of the solar heat. The idea has now been generally abandoned for reasons into which we need not here enter. But, practically, it is of no importance whether we consider the sun's heat to have been generated by the downfall of masses on his surface (continually fed by such downfall) or by the gradual contraction of the entire mass now constituting his globe, till it had assumed its present dimensions. This is the accepted form of the gravitation theory of the solar heat.

But manifestly, the greatest possible amount of heat which could have been generated in this way would be that produced by the contraction of a great nebulous mass containing all the sun's present substance, from an original extension throughout an infinitely large space to the present dimensions of the sun. It might be supposed, perhaps, that the result of such a process of contraction would be the generation of an infinite amount of heat. But in reality this is not the case, any more than it is the case that a meteoric mass allowed to fall from an infinite distance upon the sun would strike his surface with infinite velocity (after a journey lasting an infinite time—which, however, is a mere detail). We know, on the contrary, the rate at which such a mass would strike the sun—namely, about

360 miles per second. And precisely as we can calculate the velocity of such a mass after being subjected to the sun's pull over an infinitely long journey, so we can calculate the total amount of heat which would result from the contraction of the sun's mass to its present dimensions from a former extension throughout infinite space. We find that it corresponds only to about 20 million years' supply at his present rate of emission.

Thus, while the earth seems to tell us that the sun has been pouring his rays upon her at the same rate as at present during at least 90 million years, the sun seems to tell us that he has not been pouring out heat at that rate for more than 20 million years.

Even if we reject the earth's evidence, or if we endeavour to show that the rainfalls by which the earth's surface has been again and again denuded were not always due to solar heat, but may have been generated by the earth's own heat, we scarcely find our difficulty removed. For it seems utterly absurd to suppose that the mighty central orb of the solar system only attained its present activity during the comparatively recent years of the history of our earth, one of the smaller and shorter-lived members of the sun's family. Sir W. Thomson has shown, by the most satisfactory of the three methods he employed to *shorten* the estimates formed respecting the earth's duration, that more than 20 million years must have elapsed since her crust was formed—a time which certainly followed by many millions of years the actual genesis of the earth as a gaseous mass. Many physicists reject even the 400 million years given by Thomson as the superior limit, doubting whether the formulæ and data he employed could be relied upon as confidently as the various processes of mathematical calculation which he applied to them. But even if we accept his minimum result—certainly the very least which science will permit us to accept—it would still follow that the sun's present emission of light and heat could not have continued throughout the time of our earth's existence as a planet; *if* the sun's heat had its origin entirely or chiefly in those processes of contraction combined with meteoric indraught in which astronomers and physicists at present believe, *and if the space into which the sun's mass has contracted is really that which the sun we see appears to occupy.*

Mr. Croll, who passes over the latter consideration with the remark that if the sun's density increases towards the centre the supply of solar heat might be somewhat greater, suggests, as the true explanation of the difficulty, that the sun may have derived a portion of his energies in another way than merely through the process of contraction. 'In proving that the antiquity of our habitable globe may be far greater than 20 or 30 million years, we prove,' he says, 'that there must have been some other source in addition to gravity from which the sun derived his store of energy.'

He goes back to the initial state of things conceived by Laplace in presenting what is usually called the nebular hypothesis of the solar system. According to this, the whole of the solar system was formerly a great gaseous mass; but whether cold or hot Laplace did not say. As Helmholtz remarks, 'The chemical forces must have been present, and ready to act; but, as these forces could only come into operation by the most intimate contact of the different masses, condensation must have taken place before the play of chemical forces began: whether a still further supply of force in the shape of heat was present at the commencement we do not know.' Mr. Croll, who regards the chemical forces as equivalent only to a few thousand years' supply of heat, and, therefore, as comparatively insignificant, thinks we may safely infer that the original nebulous mass was intensely heated, and that in such intense heat we may find the explanation of the problem before us. 'It is evident,' he says, 'that if we admit that the nebulous mass was in a state of incandescence prior to condensation, it will really be difficult to fix any limit either to the age of the sun, or to the amount of heat which it may have originally possessed. The 20 million years' heat obtained by condensation may in such a case be but a small fraction of the total quantity possessed by the mass.'

But then the question arises, Whence did the nebulous mass derive its heat? Mr. Croll considers that we may find a satisfactory answer to this question in the assumption that the nebulous mass was formed by the collision of two bodies, each of half the mass of the sun, rushing full tilt upon each other with a velocity of nearly 500 miles per second. Their concussion would generate enough heat to last more than 50 millions of years, which we should have to add to the 20 millions of years provided for by the subsequent condensation of the mass. He asks: 'Why may not the sun have been composed of two such bodies? and why may not the original store of heat possessed by him have all been derived from the concussion of these two bodies? Two such bodies coming into collision with that velocity would be dissipated into vapour and converted into a nebulous mass by such an inconceivable amount of heat as would thus be generated; and when condensation on cooling took place, a spherical mass like that of the sun would result.'

It will be asked, Mr. Croll says (and certainly it seems likely), 'Where did the two bodies get their velocity?' It may as well be asked, he answers, 'Where did they get their existence? It is just as easy to conceive that they always existed in motion as to conceive that they always existed at rest.' At first sight it might seem a fair rejoinder to this to say that, if we are free to assign these enormous velocities to bodies in space, we must be free also to assign to them other properties such as matter can possess—heat, for instance: so that we might solve our problem at once by saying that the nebulous mass was originally supplied with enough heat to last fifty, a hundred, or a thousand millions of years. But there is a difference between motion and heat. Masses of matter might be rushing hither and thither through space for ever, without change, except when collisions occurred; whereas masses intensely hot must radiate their heat away. So that while we can, as Mr. Croll truly says, conceive the existence of bodies in motion for any length of time we please, we cannot conceive the constant existence of an intensely-heated nebulous or other mass. It *must* lose heat; whereas the bodies rushing about through space need not lose motion, and certainly would not do so unless they came into collision.

Nor is Mr. Croll's position affected by the argument that neither our own sun nor the other suns which people space are rushing about with anything like these velocities of four, five, or six hundred miles per second. For all the stars are glowing with intense light and heat, and therefore must be regarded as bodies which, like our sun (according to this theory), have been formed by mighty collisions, in which their motion was converted into light and heat. The stars, therefore, are bodies which have already lost the greater part of their original velocities, and the comparatively small velocities left them are precisely what, according to this theory, we might expect.

Yet, while an answer may be found to some of the more obvious arguments against this startling theory, it must be admitted that the theory remains surrounded by difficulties of an almost insuperable nature.

Without entering into calculations which would be out of place in these pages, we may state that the imagined collisions of bodies rushing hither and thither, even with the enormous velocities suggested, through stellar space, would resemble in frequency, or rather in paucity, collisions between bullets in an engagement between two very widely scattered parties of skirmishers. At a rate of 500 miles per second (possessed by *each*), two bodies as far apart as our own sun and his nearest neighbour among the stars, would meet each other (if their motion were suitably directed) in about 700 years. Supposing a million stars, scattered as stars are now scattered, were to rush in a flight to meet a million stars similarly scattered, at the rate just mentioned, a million years or so would elapse before the two flights had rushed through each other, and the chances would be many millions to one against even a single collision occurring. Such bodies would have to be strewn far more densely through space than the stars are to make it probable that among several millions of them one collision would occur in a million years. As the supply of light and heat resulting from each collision would not last more than 50 or 60 millions of years on the average, only fifty or sixty stars would be visible at any given moment among all those millions of bodies. So that for each star shining in that region of space (and the same reasoning applies to the whole of the stellar universe) there would be millions of dark bodies. Of these a certain proportion, probably very small, would consist of orbs which had undergone collision, had shone for 50, 60, or say 100 millions of years, and were now dead suns. The rest of the dark bodies, outnumbering the visible stars millions of times, would be bodies which had not yet encountered others after the fashion which the theory requires. These would be dangerous fellows. They might at any time come into collision either with each other, making new suns, or with suns already glowing, making these suns glow a great deal more brightly, and destroying the inhabitants of any worlds circling around them. Moreover we ought, in the course of comparatively short periods, to see such new stars suddenly begin their existence as vaporous masses glowing with an intensely bright light. Now, nothing in the least corresponding to the process of sun-formation required by this theory has ever been observed. The so-called new stars are not at all what the theory requires. They have shone with intense brightness for a few months at the outside, and have then died out; but according to the theory we require stars which shall burn with steady fires for many millions of years. Now, we might reasonably expect that for some short time following its first formation, a new sun would shine much more brightly than afterwards. Mr. Croll, indeed, supposes otherwise, his line of argument as to new stars (presently to be noticed) assuming that after a collision a star would immediately begin the steady emission of light and heat at about the rate at which it would continue to emit them afterwards: but a collision in which the supply of heat and light for 100 millions of years was generated in a moment would unquestionably produce also a great temporary outburst. Those new stars, however, which astronomers have been able to observe since the spectroscope was invented, have not behaved in the required manner. (As a friend of ours is apt to say when observation does not accord with theory, 'They didn't know, poor things, what they were expected to do.') One was found to be a star already recorded in star-maps, and has faded to its original lustre; the other, after shining for awhile as a bright star, has faded into a faint nebula or star-cloud.

Mr. Croll reasons thus as to the probable number of new stars which would be formed according to his theory:—'The formation of a sun by collision is an event that would not be likely to escape observation if it occurred within the limits of visibility in space. But such an event must be of very rare occurrence, or the number of stars visible would be far greater than it is. The number of stars registered down to the seventh magnitude inclusive is, according to Herschel, somewhere between 12,000 and 15,000, and this is all that can possibly be seen by the naked eye. Now, if we suppose each of them to shine like our sun for (say) 100 million years, then one formed in every 7,000 or 8,000 years would maintain the present number undiminished. But this is the number included in both hemispheres, so that the occurrence of an event of such unparalleled splendour and magnificence as the formation of a star, or rather nebula—for this would be the form first assumed—is what can only be expected to be seen in our hemisphere once in about 15,000 years. The absence of any historical record of such an event having ever occurred can, therefore, be no evidence whatever against the theory.' If, however, as may most reasonably be assumed, the formation of a sun in this way would be in the first instance accompanied by a most tremendous outburst of light and heat, far exceeding that which the body in its ultimate condition as a sun would emit, then we should be able to recognise the formation of any such sun within the region of space over which our telescopes range; and in that region of space there are more like a hundred million than twelve or fifteen thousand stars. An outburst ought to be recognised, on the average, about once a year; and certainly new suns are not entering on the first stage of their existence at this rate.

Moreover, apart from what we have mentioned above as to the duration of so-called new stars, what is known about one at least of the two new stars which have appeared during the last twelve years, by no means accords with what we should expect if the outburst were caused by the collision of two other suns, or of two dark masses rushing along at the rate of four or five hundred miles per second. One of them was found not to be a new star at all. It was simply a tenth-magnitude star which had suddenly acquired the brightness of a second-magnitude star. It rapidly lost its new lustre, returning to the brightness which it had had before the outburst. The other—the new star which appeared in the constellation Cygnus in November 1876—did behave in a manner reconcilable in some degree with Mr. Croll's theory. For, while no star had been known to exist where this star suddenly appeared, the new star, after shining for awhile with light resembling in character that emitted by other stars, gradually, as it lost its light, assumed a nebulous character, and is at this moment shining with light of precisely the same kind as is emitted by the gaseous masses called (from their appearance) planetary nebulæ. It would be rather rash, however, to assume that here was a case where two orbs rushing through space had encountered full tilt, and after a certain time, during which the heat excited by their collision had been reducing their substance to vapour, the entire mass had become a nebula. If we are to suppose that dark, hard masses produce suns by their collision, we enormously increase the chances against collision, because we enormously reduce the dimensions of the bodies supposed to be travelling through space. Returning to our illustration from a battle-field, it is as though each bullet were reduced in size to the thousandth part of the smallest of small shot.

There is, moreover, this inherent difficulty in the theory thus presented, that if the heat resulting from collision vapourises the entire mass, making a mighty nebula out of which in the course of many millions of years a solar system is to form, by far the greater part of this heat will be radiated away into space while the nebula is passing through the mere beginning of the process of contraction, and ages before a single member of the future solar system has assumed the form of a habitable world. The total amount of energy corresponding to the collision, if it could all be kept in stock, so to speak, till the time that the members of the system were fully fashioned, might suffice for as many millions of years as we find that our earth has actually been exposed to the rays of the sun. But there is no conceivable way in which the supply could thus be reserved till it was wanted. While a nebulous mass was contracting, it would be expending most of the heat equivalent to each successive stage of contraction. Of course, as regards the contraction due to cooling—that is, to the emission of heat—every part of such contraction would be exactly compensated by loss of heat. But the contraction due to gravitation, the only part of the process of contraction by which heat would in any sense be generated, would cause from the beginning a steady emission of heat; and whether the total rate of such emission were greater or less in the earlier stages than now, it is certain that the duration of those earlier stages would enormously exceed—say, rather, would exceed many hundreds of times—the period which has elapsed since first rain fell upon this earth, or winds blew over its surface.[1]

It appears to us that the true explanation of the difficulty (the first full recognition of which we owe to Mr. Croll) must be sought elsewhere. Apart from the fatal objection considered in the last paragraph, a

[1] This paragraph was already written when we received from Professor Kirkwood of Bloomington, Indiana, U.S. (one of the most ingenious and original astronomers of the day), a paper in which he presents the same general argument. The conclusion at which he arrives is that much the greater part of the supply of heat 'must have been radiated into space before the planets were separated from the solar mass, and consequently that the amount of geological time cannot to any great extent have exceeded the limits indicated by the researches of Sir W. Thomson.' The latter inference, as will be seen, does not appear to us to be made out; but the former seems unquestionably correct.

theory involving the genesis of all the millions of existent stars from accidental collisions among millions of millions (for fewer would not suffice) of dark masses, constantly rushing through space at the rate of many hundreds of miles per second, is not one which can find acceptance among those who are acquainted with the actual present position of stellar research. But the difficulty indicated by Mr. Croll remains to be encountered. *Somewhere* the premisses must be wrong which lead to an erroneous conclusion.

Now, we are not disposed to question the validity of the reasoning which Mr. Croll and other geologists have based on the condition of the earth's crust. The only way of diminishing our estimate of the time-interval necessary for the stratification of the earth is to assume (as we find Professor Kirkwood does) that in former ages the stratification proceeded more rapidly than at present. But, as we have already seen, this amounts really to the assumption that in former ages the sun exerted a more powerful action upon the earth than at present; and we are in no way helped, because it is the totality of the sun's action on the earth with which we are in reality alone concerned.

We revert, then, to the original proposition of the difficulty, to see whether there may not be any other way of escape. It appears that if the sun has contracted into his present dimensions from a nebula originally extending far beyond the orbit of Neptune, the supply of solar heat would not have lasted anything near the time during which we know, from the study of our own earth, that the supply has lasted. We have assumed all along that the sun's dimensions are those which the sun actually presents to the eye. May not our mistake lie here? May not the sun—or, rather, the chief portion of his mass—have contracted in reality to far smaller dimensions than he appears to possess?

Not many years ago, a question of this sort would have appeared altogether fanciful. But facts have been ascertained in the last few years which have greatly altered our ideas respecting the sun. It is quite certain that the sun we see is not the whole sun. It is, in a sense, a mere accident that we see the sun as he actually appears. If our eyesight were of a somewhat different quality, we should see the sierra which surrounds the entire globe of the sun to a depth of five or six thousand miles; thus we should see a much larger sun. With a yet further change of visual power we should recognise the inner corona, and the sun would appear yet larger. And we can quite readily conceive the possibility of the outer corona being discerned; in which case the sun would not merely appear larger, but many times larger than he is at present. It would, indeed, be possible to see the sun thus enormously enlarged without any change in our visual powers, if our standpoint were somewhat altered and (a slight but necessary detail) if we could exist under the new conditions. From the moon's surface, an observer possessing visual powers such as ours, and capable of existing without air or water, would see all those solar appendages which are concealed from our view (except during total solar eclipse) by a veil of sunlit air.

Now, precisely as it is conceivable that by a change in our visual powers or in the conditions under which we observe the sun, we might see him occupying (as he really does, for the corona is a part of him) a region of space many times larger than that occupied by the sun we see, so it is conceivable that the sun we see occupies a region of space many times larger than that occupied by the true mass of the sun. In the same sense in which we say now that the sun's volume is that indicated by the visible surface of the sun, because the mass of all which lies outside that surface is as nothing compared with the mass which lies within it, it may well be that the true globe of the sun lies far within the glowing surface we see, the entire mass of matter lying outside such much smaller true globe being insignificant compared with the mass forming that globe.

This is not a mere hypothesis, devised to meet the difficulty indicated by Mr. Croll. That it does meet that difficulty will be obvious if we consider that the difficulty depends entirely on the observed present largeness of the sun's diameter. If the diameter were one-half its supposed length, the estimated duration of the emission of heat would be doubled; if the diameter were one-third its supposed length, the duration of the emission would be trebled; and so on. The density of the solar globe would be increased in much greater degree. With a diameter reduced one-half, the density would be increased eightfold; while if the diameter were reduced to one-third its present (seeming) length, the density would be increased twenty-seven times. Now, whether it be permissible to assume that the sun's globe could have a mean density many times greater than that usually assigned to it, there can be no manner of doubt that this supposed mean density is very much less than the known conditions under which the sun's mass exists would lead us to expect. The mean density of the sun is only one-fourth the mean density of our earth, while the pressures existing in the sun's interior are thousands of times greater than those inside our earth. True, the sun's temperature is enormous, and thus an expansive power exists throughout the sun's mass which would readily overcome such contractile forces as exist within the earth's frame. But the pressures produced within the sun by gravity are so tremendous that the elastic forces of the gaseous materials of the sun's globe must be quite incompetent to resist the contractile tendency. The proof that this is so is found in the constant emission of solar heat, which represents in reality the yielding of the solar mass to the influence of its own gravitating energies.

We approach here the consideration of relations such as we are entirely unable to understand or even conceive. No experimental researches we can make can throw any trustworthy light on the condition of the sun's interior, where pressures far surpassing any we are familiar with contend with temperatures equally surpassing the fiercest heat known to us on earth. It is probable that the entire mass of the sun, whatever its real extension, is gaseous; for the heat of all the materials of that mass is greater than the critical temperature of the densest elements—that temperature at which no pressure, however great, would liquefy or solidify them. If at this tremendous temperature and at the enormous pressures to which they are exposed the constituents of the solar globe were perfect gases, there would be no limit to the density they would attain in the sun's interior. But we have every reason to believe that after a certain density had been attained under pressure, these gases would no longer behave as perfect gases, their density increasing with pressure. And we find it difficult to imagine that gaseous matter could under any pressure, however great, acquire a density exceeding many times that of the elements we chiefly see in the solid form. Yet it would be unsafe to assume any limits to the density which might be attained under constantly increasing pressure by matter maintained always at so tremendous a temperature that it was prevented from becoming liquid or solid.

If we inquire what seems suggested by the actual available evidence respecting the sun's condition, inside that glowing globular surface which conceals from us all that lies within, we find reason to believe that the sun's interior is thus enormously compressed. It can readily be shown that if the sun's mass is not thus compressed, then, rotating at the observed rate, his globe should be flattened to an extent which should be recognisable by the best methods of modern measurement. The flattening, be it understood, would still be very small. It might even escape observation, so small would it be; but the probability is that it would have been detected. On the other hand, if the sun's interior is exceedingly dense, then the flattening of his globe would certainly not be observable. Since, as a matter of fact, no flattening has been observed, the probability is that the sun is enormously compressed near the centre. It must be admitted that this part of the evidence is not very strong; but, such as it is, it bears in the direction indicated

Strangely enough, we derive from a different orb the strongest evidence on this particular point. Jupiter's mean density is the same as the sun's, if we take the visible disc of Jupiter as indicating the true size of the planet. Now it has been shown by Mr. George Darwin (from a careful comparison of the motions of Jupiter's moons with those calculated on the assumption that Jupiter's mass is not greatly compressed at the centre) that Jupiter must be very much denser at the centre than near the visible surface of his globe. This agrees with all that is known respecting that planet. We have pointed out, on former occasions, in these pages, how utterly impossible it is to explain the phenomena presented by the giant planet, on the assumption that the disc we see and measure is the true globe of Jupiter. Mr. Darwin's reasoning proves in another way that this globe lies far within the apparent outline of the planet, which in reality represents probably the region where lie the feathery clouds forming his outermost cloud-layer. Within it lie other cloud-layers, and an atmosphere of exceeding depth. Nay, it is probable that the greater part, if not the entire mass, of each of the planets Jupiter and Saturn exists at so intense a heat (though the cloud-envelopes we see are not intensely hot), that solidification and liquefaction are impossible at any pressure, however great. In this case the density of the internal parts of these planets, as of the internal parts of the sun, would be due to the vastness of the pressures exerted upon the nuclear regions.

Without insisting on this, let it simply be noted that in the case of Jupiter and Saturn it has been to all intents and purposes demonstrated that the condensation of the planet's mass is very much greater than we should infer from the apparent dimensions of the planet's globe. Since these planets are probably intermediate in condition, as they are in size, between our earth and the sun, we find another reason for inferring that the nuclear parts of the sun are exceedingly dense. If so, the difficulty which Mr. Croll has sought to deal with by imagining that not our sun only, but every sun peopling space, has been produced by the collision of formerly dark masses rushing hither and thither with inconceivable velocities, would no longer exist.

One circumstance, however, remains to be noticed. We have endeavoured to explain the apparent age of our earth's strata by an assumption which in reality implies that the sun is a great deal older than he had been supposed to be. Not merely does our hypothesis require that he should be regarded as a great deal older, but, as it has not directly enhanced our estimate of his possible total duration, it assumes in fact that he is many millions of years nearer to his end as a living sun (so to speak) than has been commonly supposed. The process of contraction, on which his vitality as a sun depends, has gone on much farther, if our theory be sound, than if we suppose the globe of the sun, as we see it, to be of uniform or nearly uniform density throughout.

But it does not seem to us that the estimate of the sun's duration which would result from our theory, would fall short of that which astronomers had formed on the hypothesis that the sun is of uniform density. (We call our view a theory, because it is based on observed facts; the usual view an hypothesis, because no one has ever ventured to assert that any facts indicate its correctness.) On the contrary, according to the usual view, astronomers had recognised a certain limiting density not very far removed from the present supposed density of the sun, beyond which the process of contraction could not probably compress his globe. According to the theory we have brought forward in explanation of observed facts, the elements composing a mass at so high a temperature and subject to such enormous pressure as the sun's may attain even in the gaseous form a density far greater than has hitherto been considered possible. Enormously though we suppose the process of contraction to have gone beyond the extent heretofore believed in, we no longer recognise as close at hand any limit beyond which that process cannot pass. For our own part, in fine, while we consider it quite possible that the nucleus of the sun may be so tremendously compressed as to correspond to a past emission of solar heat for many hundreds of millions of years, we see no reason to believe that the process of contraction may not continue with the same emission of heat as at present for hundreds of millions of years to come. It appears to us as absurd to measure the probable amount of solar energy, either already exerted in the past or available for the future, by considerations based on the behaviour of the elements at the temperatures and pressures we can obtain experimentally, as it was of old times to estimate the proportions of the heavenly bodies on the assumption that the earth is the all-important body which they were made to serve, or as it is in our own time to estimate the duration of the heavenly orbs by the minute time-intervals corresponding to the various stages of our earth's relatively insignificant existence.

THE SUN IN HIS GLORY.

Another step has recently been taken towards a more exact knowledge of the nature and condition of that mighty orb which rules and lights and warms the earth and all the family of planets. The total eclipse of the year 1878, the last which for several years is likely to be observed by scientific men, has not passed without adding notably to our knowledge respecting the sun. Other opportunities for observation, and other methods of research, have also been employed of late with considerable success. The occasion then seems a fitting one for presenting a brief and simple statement of the present position of solar research.

It is strange to consider how wonderfully our ideas respecting the sun have changed during the last quarter of a century. Twenty years or so ago, the sun was regarded by many as what Sir W. Herschel had said that possibly the sun might be—a dark orb surrounded by various envelopes of lustrous and heat-emitting clouds. According to this view, the sun might even be regarded as possibly a fit abode for living creatures. Others held a different view, the ancient but on the whole more probable opinion that the sun is a great mass of intensely hot matter. The sun-spots were known, and had been carefully watched and studied. Indeed the best series of observations ever made on the sun were either completed, or very nearly so, in the year 1858. But though much was known about the spots very little was understood. As to the physical constitution of the sun, nothing was known about it, and no one had any hope at that time that aught could be learned exactly on that point, though some few considered it barely possible that inferences of greater or less probability might be suggested by various lines of research then entered upon.

At present all this is altered—we know the sun to be infinitely more complex in structure, infinitely more wonderful in physical condition, than it was formerly thought to be. We have learned what its substance consists of, in what condition that substance exists, or rather through what varying conditions it passes. We have found the sun to be something utterly unlike the orb we see, for we have learned that even as the glowing veil of air hides by day the chief glories of the universe, so it hides the largest (though not the most massive) part even of that one sun among hundreds of millions of suns which can at that time be seen. We have also learned more exactly to measure and weigh the mighty orb which rules the motions of our earth and her fellow-planets.

The sun, as seen in the sky, is a globe of fire some eight-hundred and sixty-thousand miles in diameter, and lying at a distance from us amounting to about ninety-two and a third millions of miles. It affords a startling conception of this tremendous distance to consider that a ball fired at the sun from the mouth of an Armstrong gun, and travelling with undiminished speed directly towards him, would only reach him in about thirteen years. If the sound could travel sunwards at the same rate as in air, the sound of the explosion would reach the sun almost half a year later. An American student of science, Professor Mendenhall of Columbus, has given a striking, though fanciful, illustration of the sun's distance. If a baby had (which is not customary) an arm ninety-three million miles long, and on the first day of its existence touched the sun, then, according to the best estimates of the rate at which feeling travels, the baby might grow to manhood and the man attain to extreme old age, without ever feeling the pain of the burn. In fact, one hundred and thirty-two years would be required to convey along that monstrous arm the sensation of burning which had affected the finger tips. But, in reality, the most striking thought in connection with the sun's distance is that light, though travelling over a distance nearly equal to eight times the circumference of the earth in a single second, takes nearly nine minutes in reaching us from the sun.

In passing, a word or two may be said respecting changes which have recently been made in our estimates of the sun's distance. Many who may have remembered that a distance of more than ninety-three and a third million miles was announced in Parliament last year, as the result of the British transit expeditions, may perhaps look with doubt on the distance of ninety-two and a third million miles, which I have just mentioned. It may be well to say, then, that the official astronomers responsible for working out the transit observations have come round to this smaller distance. I ventured to express in the 'Times,' even when our chief official astronomer had stated his belief that no considerable change would be made in his result, my own opinion that the lesser distance would be eventually adopted by others if not by him. But I did not expect to find my opinion so quickly confirmed as it has been. Step by step—each two or three hundred thousand miles long —our official astronomers have reduced their estimate; until finally (at least I suppose so) they announce ninety-two million four hundred thousand miles as the most probable result of the British transit observations. They admit, as I had also ventured to point out, that any value within a million miles or so on either side of this distance can be reconciled with the observations. But ninety-two and a third million miles is the most probable value; and as six or seven different and far superior series of observations had pointed to the same distance, we may unhesitatingly accept that as within two or three hundred thousand miles of the true distance separating us from the mighty mass of the sun.

With a diameter exceeding our earth's one hundred and nine times, the sun has a surface exceeding the earth's eleven thousand eight hundred times, and a volume exceeding hers about one million two hundred and seventy thousand times. In mass or quantity of matter he does not so greatly exceed the earth. Still it would take about three hundred and thirty thousand globes like the earth to make up the quantity of matter which exists in the sun. All the planets together do not amount in mass to a seven hundred and fortieth part of the sun's mass.

The enormous quantity of matter he possesses gives the sun tremendous power, though his actual action on our earth is not so great as many imagine, for his energy is enormously reduced by distance. A child could hold a ton of matter against the pull of the sun at the earth's distance. But if this earth of ours, retaining its present size, contained as much matter as the sun, the strongest man (supposing he himself not crushed flat and thin as gold-leaf by his own weight) would not be able to lift the quantity of matter in one of our half-ounce weights. It would press downwards with as much energy as one hundred and sixty-five thousand ounces, or nearly five tons of matter, on the earth as she at present is. A small mass such as this raised only to a height of a single inch and let fall would strike the earth with three times the velocity of the swiftest express train. At the surface of the sun himself his attractive energy is not nearly so great, because his size is so much greater, and his surface so much farther away from the centre. Still a man of average weight, if placed at the sun's surface, and supposed not to be in a moment converted into thinnest vapour, would be pressed down as with the weight of twenty-six other men on his shoulders, and crushed completely flat.

Such is the sun's mass, the quality in virtue of which he bears sway over the members of his family. Mercury has to travel at the rate of nearly twenty-nine and a half miles in a second to get centrifugal tendency enough to retain his distance from the sun. Venus, farther away, requires only a velocity of about twenty-one and a half miles a second; our earth only eighteen and a half miles; Mars, fourteen and three-ninths; Jupiter about eight; Saturn six; Uranus four and a fifth; and lastly, at the outskirts of the system, so far as it is yet known, we find Neptune able to retain his distance against the enormously reduced solar attraction, by virtue of the centrifugal tendency resulting from a velocity of barely three and a third miles a second.

An as yet altogether unexplained circumstance is to be noticed with regard to the power of the sun's attraction, and indeed of gravity generally. The action of this force is exerted instantly, or, to speak more strictly, the time occupied in transmitting the action of gravity over the greatest distances in the solar system is inappreciable. Gravity cannot take so much as a second in acting over the distance separating Neptune from the sun. I cannot conveniently explain here how this is proved. (Elsewhere I have shown this, but the subject is too difficult for treatment here.) It is, however, as certain as aught within the domain of scientific research that gravity acts in this instantaneous manner. That it should do so is one of the greatest, if not absolutely the greatest, of scientific mysteries.

If the sun only ruled the motion of our earth, we should profit little from his existence. It is because by thus ruling her movements he retains her always where she can receive the necessary amount of light and heat from him, that his attractive energy is important to us. If his heat, and the light which results from it, should fail him, our earth would still continue to travel round him as at present, but she would no longer be the abode of life. There is a fine description, in Byron's 'Darkness,' of the horrors which would follow the extinction of the sun,—and though the description (as Sir J. Herschel long since pointed out) is not scientifically accurate, it is perhaps more suggestive than a less poetic but more exact account would be :— 'I had a dream,' he says,

which was not all a dream,
The bright sun was extinguished, and the stars
Did wander darkling in the external space,
Rayless and pathless; and the icy earth
Swung blind and blackening in the moonless air;
Morn came and went—and came and brought no day.
. . . . . the thrones
And palaces of crowned kings,—the huts
The habitations of all things that dwell,
Were burnt for beacons; cities were consumed,
And men were gathered round their blazing homes,
To look once more into each other's face;
Happy were those who dwelt within the eye
Of the volcanoes, and their mountain-torch.
. . . . The world was void,
The populous and powerful was a lump,
Seasonless, herbless, treeless, manless, lifeless,
A lump of death—a chaos of hard clay.
Ships sailorless lay rotting on the sea,
And their masts fell down piecemeal; as they dropped,
They slept on the abyss without a surge—
The waves were dead; the tides were in their grave,
The moon, their mistress, had expired before;
The winds were withered in the stagnant air,
And the clouds perished; Darkness had no need
Of aid from them—she was the Universe!

In reality the long miseries described so powerfully by Byron in the passages omitted from the above quotation, would not trouble the people of this earth if ever 'the bright sun was extinguished.' In less than a day every drop of moisture in the air would be precipitated. (Herschel allows two days, but I cannot see how even a day could pass without this change being completely wrought.) And then in less than another day, all the heat remaining to the black earth would be radiated away into space, and a cold in comparison with which the cold of the bitterest Arctic winter would be as the warmth of a summer's day, would take possession of the entire earth; no living thing could possibly survive to the end of the third day,—if we can call that interval a day which would pass unmeasured by the light of either sun or moon.

Among all the discoveries of modern science few are more surprising than those relating to the fires of the great central orb. When we consider merely the quantity of heat which is distributed moment by moment to the worlds around the sun, and in still greater abundance to surrounding space, we are ready almost to believe that the desolation described by Byron may be no such remote danger after all. In each second of time the sun distributes as much heat as would be produced by the consumption of eleven thousand eight hundred millions of millions of tons of coal. (If our earth's surface glowed with the same heat, she could give out as much heat as would result from the burning of almost exactly a billion tons.) But it is easily calculated that if the sun consisted entirely of coal, burning at this rate, he would burn out in less than five thousand years. It is not then by burning that he gives out heat. Again, if he were simply glowing with inherent heat and radiating that heat into space, he would lose so large a portion of his heat in five thousand years that he would be quite unfit to serve as our sun. Whence, then, is the supply maintained? and (stranger question still) whence has it been derived?

The answer is a startling one. The solar heat is derived from the gravitation of the sun's mass, leading to his steady contraction in volume. Meteoric downfall may supply, and doubtless does supply, a part of the sun's heat. But that downfall is in reality a part of the same process of contraction. The meteors as yet ungathered, which nevertheless are one day to be gathered in by the sun, must be regarded as belonging to the sun, just as clouds floating in our air belong in reality to our earth. When the sun gathers in such meteors he receives a certain accession of heat; but his volume has in corresponding degree diminished, and we know that while the contraction which he can undergo in this way, by gathering in the outlying meteoric portions of his substance, is but small, the contraction he can undergo by the shrinking inwards of his present substance is enormous; were it otherwise we could not expect a long continuance of his present emission of light and heat. But there seems scarcely any limit to the contraction he can undergo in the future.

In the past, the sun has undergone contraction far greater in amount, but less important when regarded in its heat-generating effect. For the more the sun contracts, the more effective becomes each part of the process of contraction. It is, however, certain from the study of our earth's crust, and the evidence it affords of long past ages during which the sun has poured light and heat upon the earth, that the past contraction of the sun must have been competent to produce a supply of heat such as he now emits, for a period of one hundred million years. Now here a strange difficulty presents itself. If the sun's mass had originally occupied infinite space and had contracted till it occupied—as it now seems to do—a spherical space eight hundred and sixty thousand miles in diameter, the entire supply of heat corresponding to that process of contraction would not have amounted to more than would maintain the sun's present emission for about twenty millions of years. Some have been led to believe, on this account, that the evidence given by the earth's crust must be erroneous. Others who perceive that the above-named period of a hundred millions of years cannot exceed and is probaby in reality far short of the truth, have adopted the startling theory that a large proportion of the sun's heat was derived from the collision of two suns, each travelling, with enormous velocity through space, combining to form his present mass. It appears to me that while this theory must be regarded as altogether untenable and while the evidence given by the earth's crust must be accepted as incontrovertible, we may accept, or rather we are almost forced to accept, another theory, which suggests very strange thoughts as to the actual condition of our sun. We get rid of our difficulty at once if we adopt the theory that the central part of the sun is very dense compared with the outer part, if we assume in fact that nearly all the real mass of the sun is comprised within a nucleus as small perhaps compared with the globe we see, as that globe is compared with the volume of the real sun in his glory, as total eclipses reveal that glory to our view. There are other reasons, which could not properly be considered here for regarding this view as probable. If the difficulty I have mentioned above can be removed in no other way —and I confess I see no other solution which can be regarded as even plausible—the belief in a mightily contracted solar nucleus will become a scientific necessity.

Before passing from the consideration of the sun's heat there is one thought which may well for a moment detain us. The emission of solar heat is altogether the most important process, represents altogether the mightiest energies, with which we are acquainted from actual observation. The shining of the stars at night speaks indeed of energies compared with which the energy of our sun is but as one unit among hundreds of millions. But we have no means of actually measuring the total heat emission of any star, far less of determining the totality of stellar heat and light. Solar heat we can measure; and we know of no process which can be for a moment compared with the sun's activity. Indeed, almost every other process or form of energy with which we are acquainted is a direct product of the solar energies. Now it is a strange, one may almost say a fearful thought, that the products of this tremendous and constant activity, are, according to our way of viewing the matter, almost utterly wasted.

Only one ray out of two thousand millions emitted by the sun falls on the earth; and not more than one ray out of two thousand and thirty millions falls on any of the planets. All the rest are poured into the star depths, and serve no useful purpose of which we have (at present at least) any cognisance whatever! Compared with this, no instance of apparent waste among the myriads of such instances which nature presents to our view, seems worthy of a thought. Here is the great source of all the forms of energy existing on the earth, apparently wasted in the proportion of two hundred and thirty million lost parts to one part utilised.

What the telescope has to tell us of the sun is doubtless tolerably well known to all who will read these pages. They have heard how Galileo, Fabricius, and Scheiner detected spots on the surface of the orb which had for so many ages been regarded as free from stain or blemish, an emblem of celestial purity. From the study of these the law of the sun's rotation was gradually determined—a law by no means so simple as some imagine. It is one of the strangest of all the facts known about the sun that the spots near his equator are carried round in less than twenty-five days, while those farthest from that circle only complete their circuits in some twenty-eight days. It is very difficult to picture the condition of an orb whose equatorial regions thus gain three days' rotation on regions in mid latitude in a single turning, or one complete rotation in nine or ten turnings. I believe that, when we come to regard the real mass of the sun as lying far within the limits of the globe we see and measure, this difficulty will be in a great degree removed.

If we adopt this opinion, however, the spots could no longer be regarded as belonging to the true mass of the sun. They would be phenomena affecting the globular region around his true mass, but not necessarily affecting his nuclear regions at all. We know, of course, that enormous quantities of matter must occupy the surrounding region which is limited by the visible solar surface. We are even able to determine the elements of which that matter consists. But enormous though the absolute quantity of this surrounding matter may be, it is probably exceedingly small relatively. Thus we may find an explanation of the rapid changes which affect the solar spots, and also the peculiarities of appearance which the spots present during the various stages of their formation, development, and dissolution. The singular striation of the half-tinted fringe surrounding the dark central parts of spots, might perhaps be found rather to resemble the striation of our own terrestrial and auroral curtains, than to result from an actual material striation of the sun's substance. The changes affecting sun-spots would be, like those affecting the auroral streamers, changes mainly resulting from a change of condition, not from any real change in the position of the matter forming the streamers.

However, we must not here enter into the consideration of theories or speculations, which could only be properly dealt with at much greater length and with much closer reasoning than would be suitable for these pages. Only it may be mentioned, in passing, that the theory of the auroral nature of all visible solar phenomena will be found to have its bearing on phenomena visible during eclipses of the sun, as well as on those which the telescope discloses in the solar globe as seen in the sky.

The alternate increase and diminution of the spots in number in a period averaging about eleven years and a ninth remains still among the unexplained results of solar research. We have theories in abundance to show how it might be brought about, none which seems to explain how it is actually produced. The motion of Jupiter in his orbit has been regarded as in some way associated with the sun-spot period; but since for many Jovian periods in succession sun-spots have appeared in greatest number when Jupiter was nearest to the sun, and for many such periods the greatest number when Jupiter was farthest from the sun (to say nothing of long intermediate intervals during which the greatest number of sun-spots appeared when Jupiter was nearly at his mean distance), this association must be regarded as altogether imaginary, or rather it would be more correctly described as altogether unimaginable. Other relations supposed to exist between the sun-spots and the planetary motions seem almost equally open to exception. In fact, we can only say at present that the sun-spots wax and wane in number in a period rather exceeding eleven years, with certain subordinate periods, as also some longer but less clearly recognised periods of variation: we can assert nothing positively respecting cause and causes of such periodic changes. Nay, when we examine the records of solar observation since the days of Galileo, we find reason even to question whether the period of eleven years and a ninth is the true one, or only approximately true during the present century. For in Wolff's list of years in which the sun has been most spotted and least spotted since Galileo's time, we find some intervals of more than twenty years instead of about eleven years from maximum to maximum or from minimum to minimum of sun-spot frequency.

Similar doubts seem to hang over the relation once believed to exist between sun-spots and terrestrial magnetism. This relation has always been denied by the present astronomer royal, who states that the Greenwich magnetic records afford no evidence in its favour. But it is now regarded by many astronomers as disproved in a more general way. Faye, for instance, the eminent French mathematician, asks how two periods can be regarded as associated by the very persons who maintain most strenuously the trustworthiness of researches assigning ten years as the value of one period and eleven years as the value of the other. There seems no room on the one hand for doubting the accuracy of Wolff's estimate of the sun-spot period at eleven years and a ninth. On the other there seems no reason for questioning the value of about ten years which Sabine and Lamont have assigned to the period in which the earth's magnetism oscillates in energy. Suppose, now, that at any given epoch the time when sun-spots are most numerous agrees with the time when terrestrial magnetic disturbances are greatest; then sixty years after that epoch, six times ten years having passed, terrestrial magnetic disturbances will again have attained their greatest value; but as five sun-spot periods and a half will have passed, sun-spots will be fewest in number. The time of greatest magnetic disturbance will therefore agree with the time of least solar disturbance—a relation the exact opposite of that which had prevailed sixty years before. Sixty years later the original state of things will be renewed. Sixty years later still, it will be again reversed, and so on continually. In other words, the two periods are in no way connected together.

I fear we must regard the supposed connection between the sun-spot period and magnetic disturbances, the occurrence of great auroras, and so forth, as having its origin, like so many other relations which the progress of science has caused to be forgotten, in mere coincidence. It so chanced that the sun-spot period was first fully recognised at a time when the time of many sun-spots agreed with the time of greatest magnetic disturbance for several sun-spot periods in succession. If the sun-spot period had been recognised sixty years earlier or sixty years later, the probability is that the time of *fewest* sun-spots would have been associated with the time of greatest magnetic disturbance. And if the sun-spot period had been recognised thirty years earlier or later, no relation at all would have been suspected, for the time of most sun-spots would then have come midway between the time of greatest and the time of least magnetic disturbance.

This reasoning, like Faye's, is based on the assumption that both the sun-spot period and the period of the magnetic oscillatory changes have been determined either correctly or nearly so. If they have not been, then it still remains possible that the supposed association between the two periods may have a real existence.

The determination of the actual substances, or many of them, which form the sun's mass must be regarded as among the greatest triumphs of science. In some respects it even surpasses in interest the recognition of the law of universal gravity.

Without entering into details which would here be out of place, the way in which the sun's material constitution was determined may be thus described. By means of the instrument called the spectroscope, it was found possible to separate the rays which form the sun's light into their several colours. The red rays are brought to one place, the orange rays set next, the yellow next, then the green, the blue, the indigo, and lastly the violet. Not only are the colours thus distributed, but they are arranged according to their several tints, the red merging by indefinite gradations into the orange, the orange into the yellow, and so on. Now if we imagine a number of threads of different colours thus arranged, we see that the finer the threads and the greater their number, the more perfect would the gradation be. We can readily conceive that though threads of all the tints of red, orange, yellow, &c., might not be present in the collection, it might yet happen that the entire space occupied by the array would be covered, simply because the breadth of the individual threads might enable them to cover more than the space really due to their respective tints. But if for coarse threads fine threads were substituted, for fine threads mere filaments, for these still finer filaments, and lastly such filaments as twisted in hundreds would form but a thread like that of the spider's web, we see that millions of tints might be represented in a rainbow-tinted streak a few inches in length, and yet the fine filaments composing it might barely touch each other, allowing no gaps to be seen. Such an array of tints (the threads lying square to the length of the rainbow-tinted streak) would fairly illustrate the separation of colours in a pure spectrum. But when sunlight is thus dealt with, when the countless millions of tints really forming it are brought to their proper position in the spectrum, it is found that thousands of tints are missing. It is as though from the array of fine filaments just described, forming a complete rainbow-tinted streak, hiding a dark background, hundreds of red filaments (not close together, but of distinct tints) were withdrawn, hundreds of orange filaments, hundreds of yellow filaments, and so forth, until instead of a perfect rainbow-tinted streak there remained a rainbow-tinted streak crossed (athwart its length) by multitudes of fine dark lines, representing the places where filaments had been withdrawn. Such is the solar spectrum. In other words, sunlight contains rays of all the colours of the rainbow, but of the millions of millions of tints included in these colours hundreds of thousands are missing from among the solar rays.

Now it was found that every element when in the vaporous form and glowing with intensity of heat, has the power of emitting rays of certain special tints peculiar to itself. At first no connection was perceived between this discovery and the existence of dark lines in the solar spectrum. But at length Kirchhoff discovered that each element has the power of absorbing rays of the same tints which it emits. When a vapour is interposed between the eye and a mass of glowing solid or liquid matter, the vapour allows all rays except those peculiar to itself to pass freely. But it absorbs the rays which it is capable of emitting. If it is cooler than the glowing solid or liquid matter it cannot make up by its own emission for the rays which it absorbs; thus when the light which reaches the eye is analysed with the spectroscope, these tints are found to be deficient. If it is hotter it more than makes up for the loss of these rays, and under analysis with the spectroscope these tints are found to be in excess. Lastly, if it is at the same temperature as the glowing solid or liquid source of light, its emission just makes up for its absorption, and the spectroscope affords no trace of any effect produced by the interposition of the vapour.

The application of this great discovery of Kirchhoff's to the interpretation of the solar dark lines is obvious. Every dark line or missing tint is due to the absorptive action of some vapour in the sun's atmosphere. Any family of such lines which can be shown to be identical with a family of lines which some given gaseous element emits when in a given condition, proves that that element in that condition is present in the sun's atmosphere at a cooler temperature than the mass which it surrounds. If any set of bright lines be seen in the solar spectrum—that is, if a certain family of tints appear in excess, and can be similarly identified with the special tints of a known element—we infer, or rather we know, that that element is present in the sun's atmosphere at a higher temperature than the general mass of the sun.[1]

[1] The subject of spectroscopic analysis is so difficult to explain, and so great a variety exists in the receptivity of different minds for different explanations, that I feel tempted to quote here an entirely new and original explanation given by Professor Newcomb in his fine work on Popular Astronomy:—'Suppose Nature should loan us an immense collection of many millions of gold pieces out of which we were to select those which would serve us for money, and return her the remainder. The English rummage through the pile, and pick out all the pieces which are of the proper weight for sovereigns and half-sovereigns; the French pick out those which will make five, ten, twenty, or fifty franc pieces; the Americans the one, five, ten, and twenty dollar pieces, and so on. After all the suitable pieces are thus selected let the remaining mass be spread out on the ground according to the respective weights of the pieces, the smallest pieces being placed in a row, the next weight in an adjoining row, and so on. We shall then find a number of rows missing; one which the French have taken out for five-franc pieces, close to it another which the Americans have taken for dollars; afterwards a row which has gone for half-sovereigns, and so on. By thus arranging the pieces one would be able to tell

By this method it has been shown that the sun's vaporous envelope contains iron, lead, copper, hydrogen, sodium, magnesium, cobalt, nickel, and a number of other elements, with which we are more or less familiar. The presence of oxygen and nitrogen is also strongly suspected. Indeed, the experiments of Dr. Draper, of New York, seem to demonstrate the existence of oxygen if not of nitrogen: but doubts have been raised in this country as to the validity of the evidence which he has obtained, and for the present it will be safer to regard the matter as undecided. Albeit we can entertain little doubt that these gases and all the terrestrial elements are present in large quantities in the sun; the point which remains undetermined is whether we have yet obtained valid evidence on the subject. The *à priori* probability that these elements exist in the sun, is so great that without any evidence we might feel tolerably certain that they are present.

what nations had culled over the pile, if he only knew of what weight each one made its coins. The gaps in the places where the sovereigns and half-sovereigns belonged would indicate the English, that in the dollars and eagles the Americans, and so on. If, now, we reflect how utterly hopeless it would appear, from the mere examination of the miscellaneous pile of pieces which had been left, to ascertain what people had been selecting coins from it, and how easy the problem would appear when once some genius should make the proposed arrangement of the pieces in rows, we shall see in what the fundamental idea of spectrum analysis consists. The formation of the spectrum is the separation and arrangement of the light which comes from an object on the same system by which we have supposed the gold pieces to be arranged. The gaps we see in the spectrum tell the tale of the atmosphere through which the light has passed, as in the case of the coins they would tell what nations had sorted over the pile.'

But the sun we see is only a small part (so far as volume is concerned) of the true sun. So great is his splendour, and so brilliantly in consequence does he illuminate our atmosphere, that when he seems to be shining in full glory in the heavens he is, in reality, shorn of a large portion of his true glory, so far at least as the splendour of a heavenly body may be regarded as depending on volume.

Immediately outside the globe, we see, comes an amazingly complex atmosphere, some three or four hundred miles in depth. We may say of this atmosphere that it has never been seen even during eclipses. Its existence has been demonstrated, but it is too shallow and remains in view too short a time to be actually perceived with the most powerful telescope yet directed towards it. Its existence has been demonstrated by spectroscopic analysis, as follows:—

When the moon passing over the sun's disc in total eclipse just hides the last fine thread of sunlight, there remains still in view for two or three seconds whatever vaporous matter may lie just above the solar globe. Now during this short interval of time the light received by the observer comes from this vaporous envelope. If analysed with the spectroscope, then it must show the tints belonging to the vapours which form the envelope. And if the envelope contains all or most of the vapours whose absorptive action causes the solar dark lines, then, since these vapours are for the moment shining alone, we ought to have a spectrum showing just as many bright lines as the solar spectrum shows dark lines. To use an illustration employed in an article in the 'Times' which has been attributed to myself, 'suppose that athwart a strip of dark paper millions of fine coloured threads were laid so as to form a rainbow-tinted streak (as described above), and that while all those corresponding to the missing tints of the solar spectrum were gummed down to the dark paper, these were left ungummed;' then 'if a similar strip of dark paper, having a slightly adhesive surface, were superposed on the rainbow-tinted array of threads and gently pressed down, so that when lifted again all the ungummed threads adhered to it, the first strip, thus robbed of twenty or thirty thousand tints, would fairly picture the solar spectrum; while the second strip, showing these twenty or thirty thousand threads, would fairly picture the spectrum of the solar atmosphere. One would be a rainbow-tinted streak crossed at right angles by numbers of dark lines; the other would be a dark strip crossed by a rainbow-tinted array of bright lines at right angles to its length.' The latter beautiful appearance was presented to Prof. C. A. Young (of Dartmouth College, Hanover, N. H.) when during the total eclipse of December 1870 he examined the light received from the portion of the sun invisible under ordinary conditions, lying just outside the part of his invisible disc which last disappeared. The observation, which was questioned by some less experienced in physical research, has since been several times repeated, and always with the same result. We learn, then, that outside the visible globe of the sun there is an atmospheric shell, which cannot be less than two hundred or more than five hundred miles in depth, in which are present all or nearly all the vapours whose absorptive action produces the solar spots. The vapours of iron, copper, and lead, for instance, are present in enormous but in varying quantities in that fiery atmosphere, just as in our own atmosphere the vapour of water is always present, but not always to the same extent. Glowing hydrogen is there as a fixed constituent just as oxygen and nitrogen are fixed constituents of our own air. Whether glowing oxygen and nitrogen are present in it as non-luminous (because cool) oxygen and nitrogen are present in ours, remains yet to be determined. I had partly hoped that some evidence might have been obtained on this point during the recent eclipse; but it does not appear that any attempt was made to identify the bright lines of oxygen or nitrogen on that occasion.

Above the complex atmosphere, to a height varying from five or six thousand to ten thousand miles, is the sierra, an envelope of glowing hydrogen, into which, from time to time, other vapours are poured (apparently) from the sun's interior, especially from the neighbourhood of great spots. These sometimes extend to enormous heights, forming the objects called prominences. I have said that vapours are *apparently* poured forth; for it is by no means certain that, when in any particular region of the sierra, the glowing vapour of sodium, magnesium, or other elements, makes its appearance; it has necessarily just flowed into that region. Quite probably there is merely a change of condition in vapour already present, the vapour not arriving when it makes its appearance, but then beginning to glow. Indeed, the motions which seem to take place in the sierra and in the coloured prominences are so tremendous that it is almost a relief to recognise some way of explaining the observed phenomena which would free us from the necessity of believing in such rapid transfer of matter. Cornu has recently advanced the doctrine that the changes which have been supposed to indicate the occurrence of eruptions in the sierra, and the enormous prominences which have been regarded as phenomena of eruption, are in reality due simply to electric currents, by which vast masses of gas are caused to glow with intense lustre. He points out that the rapid extension and sudden contraction or disappearance of prominences would thus be explained, without recourse to the improbable hypothesis of jets of gas having velocities of hundreds of miles a second.

It must, however, be mentioned that this explanation would not account for the spectroscopic evidence of motions of enormous velocity towards or from the observer. If this evidence is valid—that is, if the spectral lines really have shifted their position, as some spectroscopists assert—there must have been a bodily transference of the glowing gas to which they belong, in the direction of the line of sight. The change of condition, suggested by Cornu, would in reality correspond to a greater range of vibration in the molecules of the gas. The spectroscopic observation implies an apparent change in the wave-lengths, which is a very different thing, and cannot be brought about (that is, even the appearance of such a change cannot be brought about) by the change which Cornu imagines.

This point is so important that I shall endeavour to illustrate it in such a way as to make the considerations on which it depends clear to the reader. Suppose smooth water, perfectly clear, to be invisible to a particular observer, but that so soon as the water is ruffled he can see it. Then, if he observed at a distance the appearance of ruffled water, where a moment before he had seen none, he might imagine that there had been an inrush of water there, taking place with enormous rapidity. But if he reflected that a mere change in the condition of the water would account for its becoming thus suddenly visible, he would probably accept as the most probable inference the belief that a change of this kind had taken place and that there had been no influx of water. Suppose, however, that he makes an observation of an entirely different sort. Suppose he can recognise the repeated waves, and count them as they cross a certain point in his field of view. Suppose it to be known to him that if the water is undergoing no bodily transference the waves must cross that point at a certain definite rate whether the ruffling of the water is slight or great, and that, as a matter of fact, he finds they are crossing that point at a greater or less rate. Then he cannot account for *this* peculiarity by assuming any change to have taken place in the extent to which the water is ruffled. He must infer, if his observation is trustworthy, that there is a current then transferring the water bodily, waves and all, athwart the field of view, in the same direction as the waves travel if they pass the fixed point more quickly, in the opposite direction if they pass that point more slowly, than before. Now it is such evidence as this that the spectroscope gives, or has been said to give, respecting the vapours forming the sierra, and the coloured prominences. When a spectral line is shifted, the meaning is that the waves are following each other either more quickly or less quickly, according to the direction of the change. But no change in the temperature of the vapour would cause this to happen, in whatever way this change was brought about. We can only explain an observed change in the position of a spectral line by concluding that there is at the moment a bodily transference of the glowing vapour to which the line belongs, taking place from or towards the eye with enormous rapidity. On the whole I cannot think Cornu has succeeded in overthrowing the evidence on which the inference has been based that these motions take place in and near the sun, at the rate of many miles in every second of time.

The coloured prominences may be regarded as occupying a region some hundred thousand miles in depth outside the region occupied by the sierra. It is true that a region of this enormous depth is not at any time filled with the glowing prominences, and probably over the equatorial and polar regions prominences seldom attain so great a height as a hundred thousand miles, while in the spot-zones they often range far higher. Yet in the general sense in which indeed we can alone speak of the height of the complex atmosphere and of the sierra, we may say that a region of about a hundred thousand miles in height, all over the sun, is that in which the vast coloured prominences are formed and exist.

Two noteworthy discoveries have been made within the last ten years respecting the red prominences.

In the first place those over the spot-zones are found to be markedly different in character from those over the sun's equatorial and polar regions. Whereas these, though often of vast dimensions, are usually cloudlike and comparatively quiescent, those over the spot-zones often present an appearance as of being caused by mighty eruptions from the sun's interior. On one occasion, a great mass of prominence matter one hundred thousand miles in length, observed by Professor Young, appeared to have been torn into shreds by a mighty eruption from below, and Young watched the upward motion (or apparent motion) of these shreds (threads of gas from three hundred to one thousand miles in length) till the uppermost had reached a height of more than two hundred thousand miles from the sun's surface. Whether they really travelled to that height from their former position, some forty or fifty thousand miles above the sun's surface, at the enormous rate indicated by their apparent motion, a rate of more than a hundred miles per second, is open to doubt for reasons which I have already indicated. But it is certain, that when

Professor Young watched the gradual fading out of the luminous threads at their highest range above the sun's surface, there was glowing hydrogen at least one hundred thousand miles farther from the sun than hydrogen had ever been traced before in that condition. Hydrogen relatively far cooler appears, from observations made during total eclipses of the sun, to exist at greater heights. But never before or since has hydrogen glowed in that way at so great a distance from the sun's surface. In whatever way we explain this phenomenon we cannot doubt that it indicated the existence of a very unusual disturbance in that part of the sun which lay immediately under the apparently uprushing filaments. My own opinion is that probably not hydrogen, but masses of solid, liquid, or very highly compressed gaseous matter, was suddenly flung forth on that occasion, and that the long filaments indicated regions where these masses had swiftly rushed through the relatively cool hydrogen surrounding the sun.

But we have not yet reached the outermost solar envelope. Besides the red prominences, there can be seen, during total eclipse of the sun, the solar corona somewhat resembling the glory round the heads of saints in pictures. This is a solar appendage, or (we may more truly say perhaps) a part of the sun, which has attracted more attention from astronomers than almost any other during the last ten or twelve years.

It was shown first in 1868, that the corona consists in great part of matter which reflects the light of the sun. For the light of the corona examined with the spectroscope was found to contain all the colours of the rainbow ; and though the dark lines or missing tints of the solar light were not then recognised, the circumstance was very readily explained when the difficulty of the observation (at that time) was duly taken into account. If it *had* then been proved that the light of the corona is not in the main reflected sun-light, but consists in great part of such rays as come from glowing solid or liquid matter, the inference would have been that the substance forming the corona was raised to a white heat by the sun's rays. Although this did not seem altogether improbable, yet there are difficulties in the way of such an explanation. The scattered meteors travelling around the sun, and occupying (in this scattered way) the whole region of space where the corona appears, although they would be exposed to intense heat—a heat so intense indeed that bodies on our earth would become white hot if exposed to it—would be free to radiate so readily into space, whatever heat they received, that they would probably never become white hot right through. As the moon, having no atmosphere, or only a very rare atmosphere, becomes as hot as boiling water on the side turned towards the sun, but is colder than ice on the side turned away from him, so would meteors surrounding the sun be intensely hot on the side turned towards him, but comparatively cool on the side turned from him. Thus probably the average heat of each such meteoric mass would not be greater than that of red-hot iron. Nevertheless the intense heat to which the substance forming the sun's corona is unquestionably exposed must be remembered in considering the nature of this appendage. It is so great that the earth during a total eclipse receives an appreciable amount of heat from the corona, as Edison succeeded in showing with his wonderful heat-measuring instrument, the tasimeter, during the eclipse of last July.

In 1869, astronomers found that a portion of the light of the corona comes from glowing gas, for on the rainbow-tinted background of the corona's spectrum a bright green line was seen. It was at first supposed to be identical with a line belonging to iron, but this has been shown by more exact observations to be a mistake. The element whose presence in the corona is indicated by this bright line has not yet been ascertained. All we know respecting it is that it is gaseous. It does not follow, however, that the sun is surrounded by a gaseous envelope to the height of some half-million or so of miles. Quite possibly scattered portions of this gas, whatever it may be, are either formed in the sun's neighbourhood by his intense heat poured on solid masses travelling there, or are flung forth from his interior. Or possibly the luminosity to which this bright line is due has its origin in electric discharges between solid masses travelling near the sun, not in an absolute vacuum but in an exceedingly tenuous atmosphere.

During the eclipse of December 1870 the corona was for the first time successfully photographed by Willard, of Philadelphia, at Xerez in Spain, and by Brothers, of Manchester, at Syracuse. The photograph obtained by Brothers is one of the best we have, even to the present time. It is specially interesting on account of the enormous extension of the corona on one side.

In December 1871 six excellent photographs were obtained by Mr. Davis, who superintended the photographic operations of an observing party sent by Lord Lindsay to Baicull on the western shore of Mysore. Col. Tennant, at Ootacamund in the Neilgherry Hills (ten thousand feet above the sea-level), obtained six almost equally good photographs. It was, as many readers perhaps will remember, the resemblance between the six photographs of each set, and between the two sets, which finally overthrew the theory held by some that the corona is a phenomenon of our own atmosphere, or else is something 'at the moon,' as the theorists (somewhat ungrammatically) expressed their view. A view derived from these photographs is given in the latest edition of my treatise on the sun. During the same eclipse Janssen, the French astronomer, succeeded in discerning the solar dark lines in the spectrum of the corona. In other words, he succeeded in *proving* that the coronal light is in great part reflected sunlight.

The corona till last July had been regarded as an appendage of varying extent and figure, usually somewhat radiated, and occasionally throwing out streamers to great distances, but probably nowhere exceeding two millions of miles in range outwards from the sun. It had been maintained by some (as by myself) that the observed extension of the corona is simply limited by the conditions under which eclipses are necessarily studied, the real extension being far greater. In the first edition of my treatise on the sun, written and in great part printed before the eclipse of December 1870, I remarked that 'the zodiacal light indicates the existence of a lens-shaped region around the sun within which cosmical matter is profusely strewn, and that if the zodiacal light could be traced yet farther towards the sun's place, the increase of lustre [observed towards its core and near the sun's place below the horizon] would continue, and that therefore all round the sun there would be seen a luminosity corresponding precisely with the observed aspect of the corona.' In other words, I expressed the opinion, which many at the time thought rather extravagant, that the zodiacal light is but the outer part of the solar corona. Considering that the zodiacal (for so it may more conveniently be called) extends certainly some eighty million miles from the sun, while the corona had only been traced at that time to a distance of a million miles, and was not even regarded by all astronomers as demonstrably a solar appendage, it will be understood that my opinion not only seemed to many an extravagant one, but was regarded by some as a theory which never could be submitted to any valid test.

If, however, the corona could be traced to a distance of eight million miles in the direction of the zodiacal, the proof of connection between the two would be sufficient; for the zodiacal has been traced to within a distance of eighty million miles of the sun. I expressed my belief that this could be done, and showed how the difficulty should be attacked. If the bright inner corona were concealed from view by an interposed screen, protecting the eyes of the observer, the faint outer corona could, I said, be traced most probably to a much greater distance than heretofore, even if its extension into the zodiacal were not clearly recognised.

During the eclipse of 1870, Prof. Newcomb, of Washington, tried this method, but failed in obtaining any satisfactory result. The weather was hazy, his station not well suited for an observation of so delicate a nature, and the circumstances in other respects were unfavourable. In July 1878, however, Prof. Newcomb renewed the attempt at a station high above the sea level and in beautifully clear weather. He succeeded in tracing the light of the outer corona to a distance of about six degrees or some *ten million miles* from the sun. Prof. Langley, of Pittsburg (a very skilful observer of the sun), achieved a similar success. And Prof. Cleveland Abbe succeeded in tracing luminous streaks and streamers to a distance of five millions of miles.

Thus at length the gap between the corona and the zodiacal light has been completely bridged over. What was before but a probable surmise has become a demonstrated fact. The sun, we see, enormous as is his bulk, is yet, so far as volume is concerned, little more than a point within the mighty volume of which he is the nucleus. Assigning to the zodiacal the figure of a flattened spheroid (like the earth's, only the flattening is far greater) having for its greatest diameter one hundred and sixty million miles, and for its least or axial diameter about twenty million miles, the sun's volume is less than one eight-hundred-thousandth part of the volume of this mighty disc of cosmical matter. Albeit we must remember that the mass of the sun we see exceeds in yet greater degree that of all the material of the zodiacal disc, though the volume of this tremendous disc so enormously exceeds his.

Of this mighty system is the sun we see the nucleus. What lies below that glowing surface we do not know. But of what lies around it we have obtained some degree of knowledge, slowly and painfully, though much yet remains to be learned, and far more will ever remain unknown. Here, as in so many other departments of science, the saying of Laplace is justified, 'What we know is little, the unknown is immense.'

WHEN THE SEA WAS YOUNG.

We are best able to realise the fact that our earth is a globe-shaped orb, one among many such orbs peopling space, when we contemplate the wide expanse of ocean. Although the teachings of astronomy place the real figure of our earth beyond all possibility of question, it is nevertheless not readily rendered sensible to observation.

Whatever science may teach us, we usually see the earth as a generally level surface arched over by a dome of sky, which, whether clear or veiled by clouds, deceives us as to the earth's true extent and figure. Not only is this apparent shape of the sky deceptive, suggesting a somewhat flattened dome rather than the visible half of a space which, if regarded as bounded at all, should appear as bounded by a perfectly spherical surface, but the sky, seeming to spring from the visible terrestrial horizon, appears to have an arch of very limited extent. Under ordinary conditions we unconsciously regard the portion of the sky which lies next to the visible horizon as some five or six miles from us at the utmost,[1] while the part overhead seems not more than two or three miles from us. Where the air is exceptionally clear the extent of the sky-vault appears somewhat greater; but ordinarily some such conception as we have indicated is suggested respecting the size and shape of the dome which the heavens appear to form over our heads. And accordingly, when

[1] That the mind does not, in its unconscious action, attribute a very great distance to the horizon is shown by the strange illusion produced during balloon ascents. As the balloon rises the horizon seems to rise up all around the aëronaut, so that the visible portion of the earth beneath him seems to assume the shape of a vast basin. If the mind assigned its true distance to the circle where land and sky seemed to meet, this illusion would not occur; for there can of course be no doubt that the apparent rising of the horizon all round the rising balloon is due to the idea present in the aëronaut's mind that, while he rises perceptibly from the earth, the circle forming the visible land-horizon ought perceptibly to sink, which it would do if it were as near as it had been unconsciously assumed to be.

we try to realise the idea that the earth is a globe, we unconsciously picture it as a globe enclosed within the sky-vault, which we conceive as extended below the horizon so as entirely to surround the earth. According to this conception the earth would have a diameter of no more than some thirteen or fourteen miles; and reason at once rejects this conception as altogether inadequate. But where there is a wide expanse of ocean, whether partially limited or not by land-scenery, the real extent of the terrestrial globe is suggested, though not actually indicated. The mind recognises, from the appearances presented to the eye, that the ocean has a curved surface of enormous extent; while the arch of the sky is recognised as manifestly not springing from the visible horizon, itself thrown much farther away (if the eye is well raised above the sea-level) than when an ordinary land-surface limits the range of view. When the air is very clear, so that objects many miles beyond the water-horizon can be distinctly seen, the sense of the real vastness of the terrestrial globe is still more strongly impressed on the mind, especially if the objects so seen are such that their actual distance and position can be recognised. For instance, a portion of elevated land-surface seen beyond the sea-horizon does not so strongly suggest real remoteness as a ship 'hull down,' unless there should happen to be land nearly at the distance of the sea-horizon, so that by the greater distinctness of such nearer land the remoteness of other land seen above the horizon-line is indicated.[1]

[1] For the same reason an ocean scene at night is seldom so sug-

But apart from the effect produced, as it were instinctively, by the actual appearance of the ocean, another effect is produced on the mind by the consideration of the ocean's real nature. Of all terrestrial features the ocean is the one which best deserves to be regarded as cosmical. Rather, perhaps, it should be said that the division of a planet's surface into land and water is the characteristic most readily to be recognised when the planet is viewed from some other celestial orb; so that when we contemplate our ocean we are regarding a feature of the earth as a planet—one, too, whereof others besides the inhabitants of the earth may be cognisant. The thought that we may thus be sharing our impressions of the earth's condition with beings of some other world—that, in however diverse a degree, inhabitants of Venus, or of Mercury, or perhaps even of Mars, may be able to note that very feature which we are considering—brings forcibly before the mind the fact, otherwise so hard to realise, that this earth of ours is a globe travelling like the other planets round the sun, rotating on its axis as we see the other planets rotating; and that, in fine, of all those orbs which astronomy presents to us as distributed and moving so variously through space, the earth is that one which we are able to examine under the most favourable conditions. So that an astronomer

gestive of the earth's real nature as a daylight view of the ocean; for the curvature of the ocean-surface cannot be clearly recognised at night, nor usually can any objects far beyond the sea-horizon be perceived at all, still less their true distance appreciated.

at such times comes to recognise an astronomical and cosmical, rather than a merely terrestrial, interest in the contemplation of our earth. He finds his science brought into close connection with terrestrial researches, since these afford the only means available for examining one among the orbs which form the subject of his study. And although his observations may serve to render him very doubtful whether among all the orbs in space there is a single one which very closely resembles the earth, yet he finds reason also to believe that in general respects the earth's past and future condition illustrate well the significance of phenomena presented by orbs now very unlike her. So that the astronomer finds a new interest in contemplating the earth as one among the bodies to which his science relates. It is not merely with regard to space, but with regard to time also, that her aspect, thus viewed, becomes suggestive. This globe, to which we are bound by the chains of a universal force, is not only among the unnumbered and all-various globes scattered throughout infinite space, but we perceive in her the traces of processes carrying back our thoughts over unnumbered æons in the past, the germs of effects belonging to periods as immense in the remote future.

In this respect the study of the ocean is especially suggestive. For of all things terrestrial the ocean is at once the most ancient and the one which will endure longest. Mountains and hills have from time immemorial been taken as emblems of the long-lasting. The Bible speaks of 'the utmost bounds of the everlasting hills;' compares 'the precious things brought forth by the sun and moon' with the 'chief things of the ancient mountains and the precious things of the lasting hills;' and, as a supreme type of the Almighty's power, Habakkuk says: 'God stood and measured the earth; and the everlasting mountains were scattered, the perpetual hills did bow.' But, in reality, the mountains are young compared with the ocean,[1] while for ages after our present mountains have disappeared the same ocean whose waves beat now upon our shores will lave the shores of continents as yet unformed.

[1] It is related in the Life of John Herschel that when he was still a boy he asked his father, the great astronomer, William Herschel, what he thought was the oldest of all things. 'The father replied, after the Socratic method, by putting another question: "And what do you yourself suppose is the oldest of all things?" The boy was not successful in his answers; whereon the old astronomer took up a small stone from the garden-walk: "There, my child, there is the oldest of all the things that I certainly know."' The biographer from whom we have quoted says that we can trace in that grasp and grouping of many things in one, implied in the stone as the oldest of things, as forming one of the main features which characterised the habit of the younger Herschel's philosophy. But in truth the stone speaks to the thoughtful mind of something far older than itself—not, indeed, older in respect of mere existence as matter (for all matter is eternal; and in this sense the bud that flowered yesterday is no less ancient than the substance of the time-worn hill, or the waters of the everlasting ocean), but older in the sense wherein that which fashions is older than the thing fashioned. For the stone upon the garden-walk at Slough had either been rounded by the waves of ocean, or had been shaped by the running waters of brook or river formed by rains, the proceeds of evaporation from ocean's surface. Nay, even passing to still earlier periods of the stone's history—leaving, that is, the consideration of its formation as a stone to consider the formation of its substance—its substance was gathered at the bottom of the sea when the ocean was already more aged than the oldest mountains now existing.

But even those periods of the ocean's history which are thus brought before our thoughts—the vast ages during which the land-surface of the globe has been constantly changing, rising and sinking alternately according to the varying pressures exerted by the earth's interior forces, and the ages yet to come, during which like changes will take place—are as nothing compared with the duration of three stages of the ocean's history, one of which we now purpose to consider. The ocean's entire existence under its present aspect is one of these stages; of the others, one preceded and the other will follow the present stage at intervals of probably many hundred millions of years; while the waters comprising the ocean presented during the first stage, and will present during the coming, or third stage, an appearance utterly unlike that of the ocean in the present era of its existence.

It is now admitted by almost all students of science that the earth, and the solar system of which she is a member, reached their present condition by processes of development. The exact nature of those processes may be matter of doubt and uncertainty, just as the exact nature of the process of development by which animal types have reached their present condition may be doubtful. But exactly as biologists hold by almost universal consent the general doctrine of development, though they differ as to the exact course along which such development proceeded, so every astronomer of repute believes in the evolution of the solar system by natural processes, though different ideas may be entertained as to the exact history, either of the solar system as a whole, or of its various members, during long past æons of ages. Whatever theory of evolution we adopt, however, or in whatever way we combine the various theories which have been advanced, one fact in the past history of our earth stands out with unmistakable distinctness. The whole frame of the globe on which we live, and move, and have our being, was once glowing with intense heat. Whether we consider the earth's frame with the geologist, or study with the astronomer the nature of the planets' movements and the evidence so afforded respecting prior conditions of the solar system, we are alike forced to this conclusion. At a very remote period the whole substance of the earth must have been molten with intensity of heat; at a still more remote period the whole of that substance must have been gaseous with a heat still more intense; and these stages of the earth's history, remote though they were, and continuing so long that, according to our modes of measuring time, they were practically everlasting, were yet but two among a series of eras whose real number, no doubt, was to all intents and purposes *infinite.*

Now when we go back to even the nearer of those two eras we find that we must conceive of our ocean during that era as utterly unlike the seas which now encompass the earth. Its substance was the same, or nearly so, but its condition must have been altogether different. No water could for a moment rest upon the intensely hot surface of a globe raging with heat exceeding that of a smelting furnace. There could not have been during that era oceans of liquid water, though all the water of our present oceans surrounded the earth then as now. The water must at that time have existed in the form of mixed vapour and cloud; that is, it must have been spread through the air partly as pure aqueous vapour and partly in those aggregations of minute liquid globules and vesicles of water forming visible cloud-masses. There must also at that time, as now, have been various kinds of cloud-forms—an outside layer consisting of the light feathery cirrus clouds, below that a layer of the cumulus or 'woolpack' clouds, and below that again a deep layer of the densest nimbus or rain-clouds from which perfect sheets of rain must at all times have been falling; not however, to reach the glowing surface of the earth, but to be vapourised in their fall, and in the form of vapour to pass upwards again. We say that all this *must* have been; because, in point of fact, however doubtful we may feel as to many details of the earth's condition in the remote era we are considering, there can be no doubt whatever as to the general facts indicated above. We have only to enquire what would happen at the present day if the earth's whole frame were to be gradually heated until at last the surface glowed with a heat equal to that of white-hot iron, to perceive that, whatever other changes might take place, the ocean certainly would be entirely evaporated—boiled off, so to speak. But the water thus added to the earth's atmospheric envelope in the form of vapour could not possibly remain *wholly* in that form. At a great distance from the glowing earth the aqueous vapour would find a cooler region, and higher still would be exposed to the actual cold of space. Hence there would follow inevitably the formation of clouds of the various orders, *cirrus*, *cumulus*, and *nimbus*, not probably in absolutely distinct layers, but the *cirrus* commingled with the *cumulus*, the *cumulus* with the *nimbus*, and the whole series of cloud-layers affected by the most violent disturbances, partly from the continual rushing upwards of freshly-formed vapour, partly from the continual rarefactions and condensations of the air under the varying conditions to which it would be subjected through the continual changes of the watery envelope. For at every change from the form of pure aqueous vapour to the cloud-form, an enormous amount of heat would be developed, while corresponding quantities of heat would be withdrawn in vapourising other masses of watery matter. The depth of the atmospheric region throughout which these stupendous processes were continually in progress must far have exceeded the depth of the cloud-regions of our own atmosphere. For the same heat which prevented the water from resting on the earth's surface must have prevented the heavier rain-clouds from approaching within many miles of that surface without being turned into pure aqueous vapour. Again, not only would the layer of rain-clouds, thus raised many miles above the earth's surface, be also many miles in depth, but the heat prevailing throughout the layer would in turn prevent a layer of cumulus clouds from being formed, except at a great height above the rain-cloud layer. In like manner the cirrus or snow-cloud layer would be raised high above the layer of the cumulus clouds. And each of these layers, besides being separated from the next below by a deep intermediate space of commingled cloud-forms, would also be of great thickness. Hence we may fairly assume that the extreme range of the lightest and highest clouds in that era of the earth's history must have been many miles from the earth's surface, even if the atmosphere then contained no greater amount of matter (other than its watery constituents) than at present. But we have reason for believing that, besides the oxygen and nitrogen now present in the air, there must have been at that remote era enormous quantities of carbonic, chloric, and sulphurous gases besides an excess of oxygen; and all these, with the aqueous vapour (alone far exceeding the entire present atmosphere of the earth), expanded by a tremendous heat. This heavily-loaded atmosphere must therefore have extended much farther, we may even say *many times* farther, from the earth than her present aërial envelope. It is not at all unlikely that the outermost part of the cloud-envelope was then several hundred miles from the earth's surface, itself raised, through the expansive effects of heat, many miles above the level it was to assume when cooled. In attempting, indeed, to conceive the effects produced by that tremendous heat with which, most certainly, the whole frame of our earth was once instinct, we are far more likely to fall short of the reality than to exceed it, partly because the physical processes concerned are so far beyond our ordinary experience, but much more because they operated on so inconceivably vast a scale.

While it cannot but be regarded as certain (that is, as not less assured than the theory of cosmical development itself) that during a very remote and long-lasting period the water now forming our seas surrounded the earth in the form of mixed vapour and cloud, yet this consequence of the development theory, however certain, is so remarkable that one would wish to see it confirmed, if possible, by some evidence derived from actually existent worlds. Now as the various orbs peopling the universe occupy all regions of space, so they must present all the various phases through which each orb has to pass with the progress of time. It would be absurd to suppose, for instance, that every star (that is, every sun) peopling space is passing through exactly the same period of sun-life as our own sun, no less absurd to suppose that every planet is passing through the same period of planet-life, or each moon through the same period of moon-life. But it is in reality seen to be as absurd, when once we open our eyes to the real meaning of the astronomy of our day, to suppose that among the millions of millions of bodies which exist even in that mere corner of space which is measured by the range of our most powerful telescopes, there are not illustrations of *every* stage of the existence of worlds in space, from the first known to us, the vaporous, to the sun-like, and thence through all the forms of world-life down to the stage of absolute refrigeration or planetary death. Some among these varieties must exist within the solar system, and therefore admit of being telescopically examined, unless we suppose that by some amazing accident all the members of the solar system are passing through the same exact stage of world-life. But this, though it is the theory commonly accepted (because of a species of mental indolence which makes the most uniform theory appear of easiest acceptance), is in reality the most glaringly improbable, or rather the most utterly impossible, theory it ever entered the heart of man to conceive. It is as though one who knew that a number of ships, unequal in size and power, had set out at different times from various ports on long sea-journeys, should assume, as the most probable opinion respecting their position at any time selected at random, that they were riding all abreast upon the long crest of some great ocean roller.

But regarding the planets of the solar system as presumably in various stages of world-life, according to what law may we expect to find them ranged in point of age? May we take the outermost as the oldest, and the innermost as the youngest? According to the development theory conceived by Laplace, we might do so; though even then the various ages assigned to the several planets would only be arranged in the order of their actual antiquity, not with reference to the youth, maturity, and decadence of planetary life. A planet younger than another in years might be older in development; just as an animal twenty years old might be aged, while another thirty years old might scarcely have reached maturity. Moreover, it begins to be recognised that Laplace's theory of the formation of our solar system from without inwards does not present the whole truth, even if it presents the most characteristic feature of the system's process of development. Other processes have been at work, and even still continue to be at work, which may have helped to complete the fashioning of interior planets while outer planets still remained unfinished. Indeed, it is more than suspected that Jupiter may still be growing, and that Saturn may not even have assumed his final planetary form.[1] But undoubtedly the most important consideration is the first mentioned. Among planets so unequal in size and mass as those of the solar system it cannot be but that the duration of planet-life and of its several periods must differ very largely. If all the planets, then, had been fashioned simultaneously, they would now have reached very different stages of progression. Not only so, but even enormous differences in the epochs of planetary formation would probably be more than cancelled by these varieties in the rates of growth and development.

Shall we, then, take quantity of matter as the main guide for determining the relative duration of planetary life and of its various stages? Experiment will readily show whether and to what degree such a guide might

[1] Something of this sort is hinted at by Laplace himself, when he says of Saturn's rings that they seem to him to be 'des preuves toujours subsistantes de l'extension primitive de l'atmosphère de Saturne, et de ses retraites successives.'

be trusted. It is manifest that the chief question to be determined is the relative rate of planetary cooling through the various stages, from the time when a planet is a mere mass of vapour, down to the time when its whole substance is entirely refrigerated. Suppose, then, we take two globes of iron, one two inches and the other one inch in diameter, and, heating them both to a red heat in the same fire, set them aside to cool. From the result we can form an opinion whether the larger or smaller of two similar and similarly heated orbs will cool the more quickly, or whether size has little or no influence on the rate of cooling. The result of the experiment leaves us no room for doubt on this point. Long after the smaller globe has ceased to glow the larger still shows its ruddy lustre, while a still longer interval separates the time when the smaller globe can be handled from the time when the larger has cooled down to the same extent. We infer, then, that size, or rather quantity of matter, most importantly affects a body's rate of cooling. Indeed, a little consideration shows that this might have been expected. For a body can only part with its heat from its surface. Now the surface of the larger globe in our experiment is four times as great as that of the smaller, and therefore the larger gives out moment by moment four times as much heat as the smaller, when both are at the same temperature; but the larger has eight times as much matter in it as the smaller, and therefore eight times as much heat to part with, both starting from the same temperature. Naturally, therefore, since the larger, with eight times as much heat to give out, expends that supply only four times as fast, the heat supply of the larger lasts longest. We should expect the supply to last about twice as long; and, but for some minor considerations which affect the practical carrying out of the experiment, that would be the relative duration of the heat-emission from the two globes. Only of course it does not follow that the test by touch would correspond with the law here indicated, for the surface of a metal globe may be cool enough for handling while the interior is still exceedingly hot.

It is, indeed, the consideration last indicated which prevents the careful student of science from accepting as demonstrated certain conclusions which have been somewhat confidently advanced respecting the time required by our own earth for cooling down to its present condition. The experiments of Bischof, for example, upon basalt have been quoted as showing that our globe would require 350 millions of years to cool down from 2,000° to 200° Centigrade, and the process has been referred to as if it were long since completed, so that that period certainly might be reckoned as belonging to the earth's past; yet an enormous portion of the earth's globe may still possess a degree of heat between those limits, and possibly nearer to the higher limit than to the lower.

Yet while it is in our opinion an altogether hopeless task to attempt to deduce absolute time-measures, either experimentally for the determination of our earth's antiquity, or theoretically for the comparison of other planets' development with hers, we can nevertheless very confidently infer that some planets must be far less advanced than the earth towards planetary maturity, and that others must have passed beyond such maturity to extreme old age, if not to decrepitude or even to planetary death. When we consider, for instance, that the quantity of matter in Jupiter exceeds three hundred-fold that in our earth's globe, we cannot doubt that the stages of Jupiter's existence as a planet must exceed the corresponding stages of the earth's existence many times in duration. We cannot argue, indeed, directly as follows, as some have done: Since Jupiter contains three hundred times as much matter as the earth, the globe experiment described above shows that Jupiter would take nearly seven times as long as the earth in completing any given stage of planetary cooling, for if one globe contains three hundred times as much matter as another it will exceed this other nearly seven times in diameter. Nor can we proceed to argue that, since Bischof's experiments indicate 350 millions of years for one stage of the earth's cooling, Jupiter would require more than 2,350 millions of years for that stage, and so must be at least 2,000 millions of years behind the earth in development, from the consideration of that stage alone, and probably some 10,000 millions of years behind the earth altogether, in such sort that some 10,000 millions of years hence Jupiter will be in the same stage of planetary existence that our earth is now passing through. The definiteness of such statements as these makes them more attractive to many than more general statements, but they cannot be relied upon. All that can be safely alleged—and manifestly so much *can* be safely alleged—is that planets like Jupiter and Saturn, exceeding the earth enormously in quantity of matter, must have required far longer periods of time for the various stages of planetary development, and must consequently be as yet far less advanced towards planetary maturity. It follows, equally of course, that bodies like Mars, Mercury, and the Moon, as well as the moons of Jupiter, Saturn, and Uranus, being so much less than the earth in mass, must require much less time for the various stages of their development, and may be regarded as having probably long since passed the era corresponding to that through which our earth is now passing.[1] It would be, therefore, to Saturn and Jupiter that the telescopist would turn for indications of the existence of ocean waters in the state wherein our own ocean must once have existed. Instead of holding the opinion, commonly expressed in our books of astronomy,

[1] Only it is to be noted that the smaller the orbs considered the smaller the periods of their existence, and the less, therefore, the probability that differences so arising would cancel differences in the actual epoch of first formation. For instance, suppose that the above reasoning about Jupiter could be relied upon in points of detail as well as in its general sense. Then we see that a difference of no less than 2,000 millions of years comes in as affecting one stage only of the history of that planet and of our own earth; but if instead of comparing our earth with Jupiter, containing three hundred times more matter, we compared her with an orb which she *exceeded* in the same degree, we should find that the smaller orb would require about 25 millions of years for the stage which lasted 350 millions of years in the earth's case—a difference of only 275 instead of 2,000 millions of years.

that, unless very strong evidence is presented to the contrary, other planets ought to be regarded as probably like our earth, we ought (at least if we accept, as every astronomer does, the doctrine of cosmical evolution) to expect to find Jupiter and Saturn in some far earlier stage of planetary existence, and only on the strength of absolutely overwhelming evidence to admit the possibility that they may resemble the earth. Seeing, however, that every particle of evidence yet obtained respecting those planets favours the belief that they are in that early stage of development in which we should expect to find them, while many parts of the telescopic evidence are such as cannot possibly be interpreted on any other theory, it would seem to be only by an amazing effort of scientific conservatism that the old view, originally incredible and opposed by all the telescopic evidence, is retained in our books of astronomy, as though it had been the subject of some such demonstration as Kepler gave of the laws which bear his name, or Newton of the laws of gravity.

Without entering here at length into the evidence relating to the age of the planets Jupiter and Saturn, or rather to their present stage of development, we shall consider how their appearance corresponds with that which the earth must be supposed to have presented when the waters now forming her oceans enveloped her in the form of commingled vaporous and cloudy masses.

We have seen that at that remote epoch the earth must not only have been completely cloud-enwrapped, but that the outermost of her cloud-layers must have been raised hundreds of miles from her real surface. Measured, then, by an observer on some other planet, her apparent dimensions would then have been far greater than at present, for her outermost cloud-layer would be measured, not her true body. Thus judged, then, to have a much greater volume than she really has, she would be regarded (supposing her total mass to have been determined, as it might readily have been, from the motions of her moon) as having a mean density much less than that of her actual globe. How much less we do not know, because we cannot determine the extent to which her own frame would be expanded, her atmosphere swollen, and the various cloud-layers floating in it thrust away, so to speak, from her intensely heated surface. But it may well be believed that her apparent diameter would be so increased that (her volume being increased necessarily in a much greater degree) her estimated density would be much less than her present density. Now this precisely corresponds with what we find in the case of Saturn and Jupiter, each of these planets having a very small density compared with the earth's, though the tremendous attractive power residing in their enormous globes would, if unresisted, lead to a high degree of compression, and therefore to great density. The evidence afforded by the spectroscope renders it highly improbable that these planets are formed of other substances than those forming the earth, or of the same substances in very different proportions. We know that the attractive energy of these planets' masses must act out yonder precisely as the energy of our earth's mass acts throughout *her* frame. Experiments assure us that no cavities can possibly exist in the interior of a planet, so that Brewster's ingenious attempt to account for the small density of Saturn and Jupiter, by supposing these planets to be but hollow shells, fails altogether to remove the difficulty. There remains, then, only the supposition that these planets' attractive energies are in some way resisted, and the natural effect of those energies, extreme compression, prevented. And we find just the required explanation in the theory (to which we had been already led on *à priori* grounds) that these planets are still young and therefore intensely hot, the waters one day to form them being thus raised into their atmospheres, enveloping the planets in enormously deep and complex layers of mingled cloud and vapour, the planets' real globes lying far within these cloud-envelopes, and being also themselves greatly expanded by the tremendous heat with which their substance is instinct. Not only is this the only available explanation of the small density of the planets Jupiter and Saturn, but it is a manifestly sufficient explanation.

It is next to be noticed that certain very striking phenomena would result from the great depth of the earth's vapour-laden and cloud-laden atmosphere, disturbed not only by tremendous hurricanes moving horizontally, but also by vertical movements of great energy and velocity. Conceive the descent of vast sheets of water towards some intensely-heated portion of the earth's surface, and the effect of their rapid conversion into vapour. The mass of vapour thus formed, being much lighter than the surrounding atmosphere, would rise just as heated air from a chimney rises in the surrounding cooler and therefore heavier air; only with much greater rapidity, because the vapour of water is far lighter than heated air, and the atmosphere of the remote period we are considering was far denser than our present air. The mass of vapour would rush upwards to an enormous height in a very short time, and, coming from a region relatively near the centre of the earth to a region farther away, it would be affected by the difference in the rate of rotational movement at these different levels. For instance, at the present surface of the equator the movement due to rotation has a velocity of rather more than a thousand miles an hour, while at a height of a hundred miles above the surface the air is carried round with a velocity twenty-five miles greater per hour. If, then, a body or a mass of vapour were shot upwards from the equator to a height of a hundred miles, it would, while at that height, lag behind the surrounding parts of the air, and, in fact, would travel backwards at the rate of twenty-five miles an hour.

If the matter propelled upwards were vaporous, and when at the higher level became condensed into cloud, a trail of clouds would be formed along a latitude-parallel, and, as observed from some other planet, the earth would appear to be girt round by a whitish band parallel to the equator. The deeper the envelope of mixed vapour and cloud, the more readily would such bands form; and remembering the tremendous energy of the causes at work, the whole frame of the earth glowing with intensest heat, and keeping the whole mass of water now forming our oceans in the form of mixed cloud and vapour, we cannot doubt that well-marked belts must almost at all times have existed in the earth's cloud-envelope. The earth, then, would have appeared as *a belted planet*, resembling the planet Jupiter (or Saturn without his rings), but on a miniature scale. It is, indeed, common enough to find the belted aspect of Jupiter and Saturn compared with the probable present aspect of the earth, because of the existence of a zone of calms near the equator, bounded on the north and south by the trade-wind zones, and these in their turn by the zones of the counter-trades. But there is not the slightest reason for supposing that these so-called zones could be recognised by an observer viewing the earth from without. Still less reason is there for supposing that they would, even if recognisable, resemble in the remotest degree the well-defined bands surrounding the globes of Saturn and Jupiter. Such as they are, too, they would be found obeying the influence of the sun as the ruler of the day and also of the seasons; they would be also limited to sea-covered regions; and, in fine, they would correspond much more nearly with the appearances presented by the planet Mars (where occasionally for a few hours portions of bands, not complete zones, are seen across the

Martian seas) than with anything shown on the discs of Jupiter and Saturn. What we see on these giant planets corresponds closely, however, with what we should expect to find in the case of planets whose vapour-laden and cloud-laden atmospheres are so deep as to form a considerable portion of the disc seen and measured by astronomers. For the belts of these giant planets show no dependence whatever upon the progress of day and night, or of the long years of Saturn and Jupiter, but behave in all respects as if generated by forces residing in the planets themselves; their well-defined shapes also corresponding exactly with what we should expect from the mode of formation indicated above.

But, returning to the earth, it is manifest that cloud-belts formed in the way we have described would not be permanent. Sometimes they might continue for several weeks, sometimes perhaps even for months; but frequently they would be formed in a few hours, and last but for a few days, or not even, perhaps, for an entire day. So that the belts of the planet earth, viewed in those times from some remote world, would present changes of appearances, sometimes occurring slowly, sometimes rapidly. Now this precisely corresponds with what is observed in the case of the belted planets Jupiter and Saturn. Sometimes the belts remain, though undergoing constant changes of form, for weeks or months together, while sometimes they vanish very soon after their formation.

Again, it is clear that other changes than the formation or dissipation of cloud-belts would affect the deep cloud-laden atmosphere of the planet. Hurricanes and tornadoes would rage from time to time, and sometimes for long periods together, in an atmosphere where processes of evaporation and condensation, with all the rapid variations of temperature occasioned by them, were continually taking place on a scale compared with which that of the most tremendous tropical storm on the earth in our time is utterly insignificant. The effects of such hurricanes and whirling storms would be visible from without through the displacement of the great cloud-masses forming the belts. Sometimes cyclonic storms would produce great circular openings in the cloud-belts, through which the darker depths below would be brought into view. These openings would be visible from without as dark spots on the lighter background of the belts. At other times the uprush of columns of heated vapour, condensing as soon as it reached the higher regions of the planet's atmosphere, would cause the appearance (to an observer outside the earth) of rounded masses of cloud, which, because of their strong reflective power, would seem like spots of white upon the background even of a light belt, and show still more markedly if they appeared above one of the dusky bands corresponding to lower cloud-levels. And besides changes due to great disturbances and rapid movements in the cloud-envelopes, the changes resulting from evaporation and condensation proceeding quietly over extensive portions of these cloud-regions, would be discernible from without. The observer would see dark spaces rapidly forming, where some higher cloud-mass which had been reflecting the sun's light brightly, evaporated, and so allowed part of a lower cloud-layer to be seen. Where the reverse process took place, large masses of transparent aqueous vapour rapidly condensing into cloud, the formation of bright spots would be observed. How closely all this corresponds with what now takes place in the deep vapour-laden atmosphere of Jupiter, will appear from the following account by South of the appearance and rapid disappearance of an enormous dark spot on one of the belts of Jupiter :—' On June 3, 1839, I saw with my large achromatic, immediately below the lowest [edge] of the principal belt of Jupiter, a spot larger than I had seen before ; it was of a dark colour, but certainly not absolutely black. I estimated it at a fourth of the planet's " longer " diameter. I showed it to some gentlemen who were present ; its enormous extent was such that, on my wishing to have a portrait of it, one of the gentlemen, who was a good draughtsman, kindly undertook to draw me one ; whilst I, on the other hand, extremely desirous that its actual magnitude should not rest on estimation, proposed, on account of the scandalous unsteadiness of the large instrument, to measure it with ' a telescope five feet in length. ' Having obtained for my companion the necessary drawing instruments, I went to work, he preparing himself to commence his. On my looking, however, into the telescope of five feet, I was astonished to find that the large dark spot, except at its eastern and western extremities, had become much whiter than any of the other parts of the planet, and' in thirty-four minutes from the first observation, 'these miserable scraps' (that is, the two extremities of the original spot) 'were the only remains of a spot which, but a few minutes before, had extended over at least 22,000 miles.' Again, Webb, in his singularly useful little treatise, 'Celestial Objects for Common Telescopes,' thus describes certain small whitish spots seen for a time on the planet's dusky belts :—Recently, 'minute white roundish specks about the size of satellites' have been seen 'on the dark southern belts. Dawes first saw them in 1849; Lassell in 1850, with his Newtonian reflector, two feet in aperture. Dawes has since given several striking drawings of them,' and they have been seen with a nine-inch telescope by Sir W. K. Murray, in Scotland. 'They are evidently not permanent. Common telescopes have no chance with them, or with similar traces which Lassell has detected (1858) on the bright belts.' But, indeed, many pages might be occupied with the account of appearances on Jupiter's belts, indicating the progress of changes such as could not be looked for except in the case of a planet enveloped by an exceedingly deep atmosphere laden with enormous masses of cloud and vapour. In the case of Saturn such appearances are less often and less clearly recognised, doubtless because the planet lies so much farther away. For it should be remembered, in comparing the accounts which observers give of the two planets, Jupiter and Saturn, that these orbs are studied under very different conditions, a telescope nearly twenty times as powerful being required to show Saturn as to show Jupiter with equal distinctness.

One circumstance seems to us to merit attention here, of which, so far as we know, no explanation has ever yet been attempted. There is sometimes to be observed along the belts of Jupiter, and in particular along the great equatorial belt, a certain regularity of marking, giving to the belt affected by it somewhat of the appearance of a ring marked with a series of regular elliptical mouldings; or, to use Webb's description, the belts throw out dusky loops or festoons, 'whose elliptical interiors, arranged lengthwise, and sometimes with great regularity, have the aspect of a girdle of luminous egg-shaped clouds surrounding the globe.' 'These oval forms,' he proceeds, 'which were very conspicuous in the equatorial zone (as the interval between the belts may be called) in 1869–70, and of which the vestiges still remain (in 1872-73), have been seen in other regions of the planet, and are probably of frequent recurrence. It is by no means easy to assign a reason for this prevalent configuration, which sometimes shows itself in a solitary ellipse, seen by Gledhill and Mayer in 1869–70.' Several considerations suggest themselves when we study these peculiarities thoughtfully. First, the enormous size of these oval cloud-masses indicates that they are formed in a very deep atmosphere—they have a length and breadth often of nine or ten thousand miles, and sometimes (as in the case of the great solitary oval seen by Mayer and Gledhill) the extreme length of an oval cannot, after every allowance for possible exaggeration in the drawing, be computed at less than 30,000 miles. The regularity of their shape indicates that they are due to the operation of some cause at work below, and whose action, extending all around some central region, leads to a regular form, having, like the oval, a centre of symmetry. But the enormous size of the ovals indicates that the centre of disturbance must lie very deep down. One cannot, indeed, fairly estimate its probable depth at less than thousands of miles. Now, if we ascribe each of the oval clouds, seen when a belt looks like a girdle of egg-shaped mouldings, to a region below the cloud-stratum, we should have to suppose a girdle of such regions; in other words, that the real surface of the planet was not only zoned by such regions of disturbance, but the zone divided regularly up into equidistant regions of disturbance alternating with regions of calm. This theory is not only improbable in itself, but, since we have seen that the existence of belts of cloud arises from the lagging of cloud-masses thrown up from lower depths, we perceive that there is no reason for supposing the real surface of Jupiter to be divided zone-wise, still less for supposing the zones to be at any time divided regularly along their length. The cloud-masses lying along different parts of a zone come thus to be regarded as owing their position, not to the position of the region of Jupiter's real surface immediately underlying them, but to the *time* when the vapours forming them were carried upwards from the neighbourhood of the true surface. A regular series of oval cloud-masses, then, would be explained simply as a series which had been formed over one and the same part of Jupiter's true surface, but at successive equal intervals of time, the causes leading to the upthrowing of the vapour being alternately active and quiescent. Now, we know that such uniform, or nearly uniform, alternation of activity and rest is a phenomenon frequently to be observed in terrestrial phenomena, and very readily to be explained. For the energetic action of any particular process in nature will bring about, by its very energy, the action of the reverse process, which, again, will bring the former into work, the two alternating with gradual diminution of intensity, just as a pendulum swung in one direction is by that very motion caused to swing in the opposite direction, then back again, until gradually the alternate motion is brought to an end.[1] So that this explanation of the occasional regular disposi-

[1] We see an interesting astronomical illustration of such alternate action in the formation of successive envelopes around the head of a comet. These are generally seen to be arranged with great uniformity, envelope within envelope, separated by well-marked interspaces of transparent matter; and they rise gradually from the nucleus, the outer envelopes disappearing, and new envelopes forming within. Now, the formation of the visible envelope implies a process of one kind (possibly condensation), while the transparent space between indicates a process of the reverse kind (possibly evaporation); so that the regular arrangement of envelopes and spaces shows that there must be an alternation of these processes at nearly uniform intervals. And though the forces causing either process are, so far as we can perceive, at work all the time, we can quite readily understand how first one, then the other, prevails, each by its very prevalence for a while bringing about conditions favourable to the prevalence of the other.

tion of enormous oval cloud-masses in a zone girdling the whole frame of Jupiter, while corresponding well with conclusions to which we had been already led, is far simpler and better in accordance with observed phenomena than the idea of a series of equidistant centres of disturbance around a zone of Jupiter's real surface. It should be added, as in our opinion placing the real nature and method of formation of Jupiter's belts beyond a peradventure, that the cloud-surface in different latitudes of the planet's globe turns round at different rates, the equatorial portion moving fastest. This, of course, could not be the case if we saw anywhere the real surface of the planet, or even if the depth of its atmosphere were small in proportion to the planet's apparent diameter.

Next we may note yet another remarkable feature which the earth must have presented to observers on other worlds during the first stage of our ocean's history. With an atmosphere so deep as she then had, in which many layers of cloud were floating at various depths, it could not but happen that from time to time such changes would take place, either by the rapid appearance or by the rapid disappearance of extensive cloud-masses at high levels, that her shape would seem to be distorted. Indeed, this is only supposing that from time to time high cloud-layers formed or vanished in a part of the earth's atmosphere chancing at the moment to form a portion of the *outline* of her visible disc, instead of forming part of a belt in the mid portions of the disc. Accordingly, to an observer viewing the earth from without, her shape would not always appear perfectly circular, or rather of that figure almost circular, but very slightly elliptical, which in those remote times, as now, must have corresponded to the proportions of her real globe. Cloud-layers floating very high in the earth's extensive atmosphere would cause her disc to bulge out slightly but perceptibly, if they chanced to be so placed as to form the outline of that disc, while regions where for a while the higher layers were wanting would (under the same circumstances) appear slightly depressed below the mean outline of the disc. It might very well happen that these irregularities would usually be too minute to be detected; that effect called irradiation, which slightly expands the apparent outline of every bright object seen on a dark background, would go far to hide such peculiarities. Yet sometimes they would be too marked, probably, to escape notice, supposing only the observer's station were well placed for the observation of the earth; as, for instance, if at that remote time there were creatures living on the moon, and able to examine the earth from that convenient distance. Especially when it chanced that raised portions of the earth's outline lay between two depressed portions, or a depressed portion between two raised portions, the observer would have a good opportunity of recognising the irregularity so resulting. He would perceive in one case that the outline had two somewhat flattened parts with a sort of corner between them, while in the second case there would be flattening between two corners. Of course, in neither case would the corners or the flattened parts be well marked; they would, in fact, only be just discernible by the most scrutinising observation. It might, however, have happened at times that whole zones of cloud-layers would lie higher than usual, while adjacent to them were zones where only the lower cloud-layers were formed for the time being. During such periods the whole disc would appear out of shape, at least to very keen vision.

Now, precisely such peculiarities have been recognised in the case of Jupiter and Saturn, the two planets which, as already seen, we should expect from *à priori* considerations to be in the cloud-enveloped condition, and whose exceedingly small mean densities compel us either to believe that they are so, or else to adopt the conclusion that they are framed of materials quite different from those constituting our own earth. For that careful observer Schröter, the contemporary, and in some orders of observation the rival of Sir W. Herschel, notes that at times he could not but suspect that the outline of Jupiter was imperfectly rounded, being in places slightly flattened.[1] In the case of

[1] It may, indeed, be noticed as remarkable that such a peculiarity, if it exists, has not been more commonly observed; but in reality it would be very readily overlooked and might even be altogether imperceptible with many telescopes superior to Schröter's. It was but a few years ago that certain irregularities of the moon's surface, so extensive as to modify her outline when they chance to be so placed as to form part of it, were detected by Mr. Cooper Key, though the moon must quite often have been observed at times when the peculiarity should have been noticed; and he detected the peculiarity by a process corresponding in fact to the spoiling of his telescope, at least temporarily. It was a silvered-

Saturn, not only have occasional local irregularities been noticed, but the planet has sometimes been observed to be for a time quite markedly out of shape, bulging out in the regions corresponding to the earth's temperate zones, and compressed (relatively) in the equatorial and polar regions. It would be easy to dismiss such observations as due to optical illusion if they had been made by mere amateurs. But Schröter was no amateur telescopist: few ever surpassed him in skill, and none in zeal and patience. The peculiarity in Saturn's figure, again, was first observed by Sir W. Herschel when at the height of his fame as a telescopist; and it has since been observed by such astronomers as Sir J. Herschel, Airy, the Bonds of Harvard (than whom no better observers ever lived), Coolidge, and many others, while the practised and certainly not imaginative workers at Greenwich Observatory have recorded, in the account of their year's work, that 'this year Saturn has from time to time assumed the square-shouldered aspect.' It is impossible to reject such testimony, though beyond all question the *normal* condition of Saturn is not the 'square-shouldered,' as some have supposed. It is certain, from multiplied observations and measurements, that Saturn usually presents the

glass reflector; and he removed the silvering, so that the glass itself reflected the rays, but much less perfectly, of course, than the polished silver. He thus had a much fainter image of the moon, and, the effects of irradiation being removed, the flattening at the edge of the disc could be recognised. It is so great, when the moon is in one particular position, as to give two flat edges which would form sides of a twelve-sided polygon if the rest of the disc's outline were similarly shaped.

figure of a perfect ellipse, flattened like the earth at the poles, but in far greater degree. It is equally certain, therefore, that the square-shouldered aspect is but an occasional peculiarity. It is explained quite simply and naturally when we regard Saturn's real globe as deep embosomed within his cloud-laden atmosphere —a view of the planet (we again and again repeat) which *à priori* considerations, as well as his exceedingly small apparent density, absolutely force upon us. On the other hand, those who reject as utterly incredible, or at least sensational, the belief that the giant planets are passing through a stage of planetary existence through which our earth has certainly passed, insisting on regarding all the planets as in the same stage of their existence notwithstanding the enormous *à priori* probabilities against such a supposition, are not only compelled at the very outset to adopt the opinion that Saturn and Jupiter must be formed of materials altogether unlike those constituting our earth—a view much more opposed to their theory of general resemblance than the one we have here indicated—but when observations such as those we have been describing are brought under their notice they are compelled either to reject them as optical illusions (an explanation which will account for anything), or else to adopt the conclusion that disturbances have taken place in the solid framework of a planet compared with which the most tremendous earthquakes would be the merest child's play. Thus their very preference of observation to theory, and of the ordinary to the sensational, forces them in this case either to reject multiplied observations as mere illusions, or to adopt a theory of planet disturbance which is not sensational merely, but utterly extravagant and incredible.

When our earth's deep atmosphere bore the waters of her present seas floating aloft in the form of vast cloud-masses above her fiery surface, a remarkable peculiarity of appearance must occasionally, though perhaps only as a rare phenomenon, have been observable. Suppose that while a telescopist on Venus or Mercury was contemplating the earth, one of those rapid changes described in the preceding part affected cloud-layers forming the earth's visible outline at the moment of observation. The earth's apparent figure would then not only be distorted by the change, but the actual progress of the change would take place under the observer's eye. Most probably no change of the kind could have been detected by direct observation, many circumstances with which telescopists are familiar rendering an observation of the kind peculiarly difficult. But supposing the observer to have watched the earth when the moon was about to pass in transit across her face, and that the moon appeared at the moment close to that part of the earth's outline where such changes were taking place; then it would be possible, on account of this favourable conjuncture, to recognise the change of outline. For instance, if the apparent outline chanced to be raised above its usual position when the moon was very close, the two outlines—that of the moon and that of the earth—would seem to be in contact before they really were; but if, just at that time, the high cloud-layer which formed the raised part of the earth's outline were rapidly to disappear, then her outline would shrink in that place, and no longer appear to touch the moon's. Or again, it might happen that an observer of the moon, watching the great globe of the earth as it moved over the star-strewn heavens, would see its outline pass over and conceal some conspicuous star, but in a few minutes perceive the star reappearing outside the same part of the earth's outline. The observer would then know that the outline must have shrunk. In these and like ways observers outside the earth might in those remote times have seen the evidence of very active processes of change taking place in her deep cloud-laden atmosphere.

Now appearances such as these cannot be expected to occur frequently in the case of Jupiter or Saturn. The changes themselves which could alone produce them are infrequent, and the conditions under which the changes could alone be detected occur but seldom; so that the chance of a change occurring just where and when it could be detected are very small indeed. Yet in one case certainly astronomers have detected just such a change in the outline of Jupiter. It would be difficult—nay, we venture very confidently to say that *it is impossible*—otherwise to explain what is described by the late Admiral Smyth, one of the most careful and skilful of modern astronomers: 'On Thursday, June 26, 1828,' he says, 'the moon being nearly full and the evening extremely fine, I was watching the second satellite of Jupiter as it gradually approached to transit its [the planet's] disc. My instrument was an excellent refractor, of $3\frac{1}{4}$ inches aperture, and 5 feet focal length, with a power of 100. It appeared in contact at about half-past ten, by inference, and for some minutes remained on the edge of the limb' (that is, on the outline of the disc), 'presenting an appearance not unlike that of the lunar mountains coming into view during the first quarter of the moon, until it finally disappeared on the body of the planet. At least twelve or thirteen minutes must have elapsed, when, accidentally turning to Jupiter again, to my astonishment I perceived the same satellite *outside the disc.* It was in the same position,' as to level, 'where it remained distinctly visible for at least four minutes, and then suddenly vanished.'

This narrative is so surprising, even when explained in the simple manner which our theory of Jupiter's condition suggests, and still more so on the usual theory of Jupiter's condition, that it may be well to pause for a moment to inquire whether there may not have been some mistake. Admiral Smyth was a skilful observer, as we have already stated. His statement alone would have great weight. Still one may admit the bare possibility of an optical illusion, similar to what is described in Brewster's 'Natural Magic,' the satellite seen after the immersion being a mere trick of the mind, a 'blot on the brain which would show itself without.' Smyth himself supposed so, for he says:

'As I had observed the phenomena of Jupiter and his satellites for many years, without any remarkable irregularities, I could not but imagine that some optical or other error prevailed, especially as the satellite was on this' (*i.e.* the hither) 'side of the planet.' And probably the phenomenon thus dismissed by Smyth himself would not have been heard of, but for the fact that two other observers chanced to witness it. 'A few days afterwards,' proceeds Admiral Smyth, 'I received a letter from Mr. Maclear, Biggleswade, informing me that he had also observed the same, but that he had considered it a "Kitchener's wonder"' (old Kitchener, the telescopist, having been apt to recount every optical illusion by which he was perplexed as a real phenomenon). 'And about the same time,' adds Smyth, 'Dr. Pearson, having favoured me with a visit, asked me whether I had noticed anything remarkable on the 26th; for that he had, in accidentally looking at Jupiter, *seen the second satellite reappear!* Here, then, were three observers, at distant stations, with telescopes of different apertures, all positive as to the extraordinary deviation from rule. It may be borne in view that Biggleswade is twelve miles from Bedford' (the place of Smyth's observatory; and South Kilworth, Dr. Pearson's residence, is thirty-five). Mr. Maclear's telescope was rather smaller than Admiral Smyth's; while Dr. Pearson's was a much more powerful instrument, twelve feet long, and nearly seven inches in aperture. 'Explanation,' calmly remarks Mr. Webb, in speaking of this phenomenon, 'is here set at defiance;

demonstrably neither in the atmosphere of the earth nor Jupiter; where and what could have been the cause? At present we can get no answer.' But it is not the part of the true student of science thus to resign the attempt to explain a phenomenon merely because it is unusually perplexing. In this case we can reason directly from the observed fact to its interpretation, apart from those *à priori* considerations which in the present essay have led us to regard such a phenomenon as one to be looked for in Jupiter's case. First, the observation was certainly not an optical illusion, for three persons made it independently; secondly, it was demonstrably not due to terrestrial atmospheric causes, for it was seen from three stations far apart; thirdly, it was demonstrably not caused by any action of Jupiter's atmosphere on light proceeding from the satellite, for the satellite was between Jupiter and the observer; fourthly, the satellite cannot really have stopped, gone back on its path, and then resumed its onward course, unless the laws of nature were suspended—a theory we may dismiss in a scientifie inquiry; for a similar reason, fifthly, we may dismiss the idea that the whole mass of Jupiter moved in abnormal fashion. There remains only one possible interpretation—viz. that the outline of Jupiter's disc had changed in position; in fact, in whatever way we explain *how* this happened, the observations may be regarded as proving unmistakably that it *did* happen.

Now the supposition that Jupiter's outline altered leaves us still much to wonder at. For let us consider the extent of change necessary to account for what was observed. Smyth may have been mistaken as to the time intervals he mentions in his account, since he does not seem to have taken them from the clock. The interval, which he supposed to have lasted twelve or thirteen minutes, may in reality not have lasted more than five or six; and the time during which, after reappearing, the satellite continued visible, may not have lasted more than two minutes instead of four, as roughly estimated. But, taking only eight minutes as the total interval between the first and second disappearance, we have to account for marvellous changes in the apparent position of the planet's outline. For in eight minutes the second satellite would travel about 4,000 miles, and the outline of Jupiter must have changed by that amount, seeing that at the first disappearance the visual line to the satellite just touched the planet's apparent edge, while at the second disappearance the visual line to the second position of the satellite, 4,000 miles from the first, touched the planet's edge in its now changed position. Probably the difference was even greater; Smyth's own estimate of the time would make it at least 8,000 miles: but 4,000 miles will be enough to deal with. It is not necessary to suppose that the planet's apparent outline, *as ordinarily seen*, shrank inwards by the whole of this amount. More probably the outline bulged beyond its normal position at the time of the first disappearance, and presently shrank below its normal position, bringing the satellite again into view, and remaining thus depressed until the second disappearance had taken place. We may suppose, then, that at the beginning the surface forming the apparent outline was (at the place where the satellite's transit began) about 2,000 miles above the usual mean level, while afterwards it was much below that level. Two thousand miles being less than the fortieth part of the diameter of Jupiter, we can readily understand why even so enormous an apparent expansion or contraction should not have noticeably affected the symmetry of the planet's apparent figure. Indeed, with ordinary telescopic power the outline of Jupiter is so expanded by irradiation, that much greater changes of level would be so far masked as to escape attention. But we are not greatly concerned to reason at this stage as though the theory that the planet's outline changed required to be defended against objections. For it is absolutely certain that the outline must have changed. The visual line to the satellite certainly passed several thousand miles nearer the planet's centre at the time of the first disappearance than at that of the second, yet in both cases touched the apparent outline, which must therefore have shifted by as many thousands of miles, unless the satellite itself had stopped and retreated, or the whole bulk of the planet had shifted; neither of which events could occur except by a miracle. Now the changing of the outline, though marvellous, is not miraculous, and, being demonstrably the only non-miraculous interpretation of the observed event, must be accepted as the true interpretation—the event itself, observed as it was by three skilled astronomers, having certainly occurred.

This being so, the outline of Jupiter having certainly changed for awhile on that particular occasion, which theory, we would ask, should be rejected as fanciful and sensational—the ordinary theory, according to which the solid crust of Jupiter must, after rising 2,000 miles at least, have sunk through 4,000 miles? or the theory that a cloud-layer, floating at least 2,000 miles above the usual level of the highest visible cloud-layer of Jupiter, melted quickly into the form of invisible vapour, and thus a layer lower than usual by as many thousand miles came into view, forming for the time the planet's apparent outline in that place? According to the first theory, a surface much larger than the whole surface of our earth sank through a depth greater than the whole distance from the earth's surface to her centre. The intense heat which is regarded with such disfavour by followers of the old-fashioned ideas (really based on the Ptolemaic astronomy), if it had had no existence before, would have been generated by so tremendous a downfall, which indeed could not have taken place without vulcanian heat, exceeding in intensity what the other theory presents as the natural consequence of Jupiter's mode of formation. According to this second theory, the rising of the cloud-layer even to so great an elevation as 2,000 miles above the usual level of the highest Jovian clouds, was an exceptional phenomenon indeed, but by no means incredible; while the rapid dissipation of the cloud was not only quite easily to be explained, but corresponded with changes which have been observed to take place among cloud-layers seen on the disc itself. If a vast cloud-layer can disappear in a few minutes from view, above one part of the planet's surface, so also it can above another. One part may chance to lie on the visible disc of the planet; another may chance to lie on the edge of the disc; for these parts of the disc only bear relation to our point of view, not to the planet itself; and while a change occurring in one part would make a belt or spot seem to form or disappear, one occurring in the other position would make the apparent outline of the planet seem to bulge or shrink, as the case might be. Nay, we may add one consideration which would render the dissipation of a high cloud-layer in the position where Jupiter's outline appeared swollen, even more naturally to be accounted for than the often observed dissipation of a cloud-layer on the disc itself. For the cloud-layer which vanished on that occasion had just been carried into sunlight by the planet's rotation; and we can readily understand how the solar heat, slight though its effects may be compared with those of Jupiter's own internal heat, might bring about the dissolution of a cloud-layer which chanced to be in that critical stage where a slight cause will bring about either rapid formation or rapid dissipation of visible cloud.

The chief difficulty, of course, in the theory, or rather the most surprising result of the demonstrated fact that Jupiter's visible cloud-layer thus changed, resides in the enormous depth we have to assign to the cloud-supporting atmosphere. We have already shown in these pages[1] that, *cœteris paribus*, the atmosphere of Jupiter would be much shallower—layer for layer—than our earth's, simply because the planet's mighty attractive power would more strongly compress it. That it is manifestly not thus compressed indicates, as we then showed, the intensity of the heat pervading its whole extent. But that it should range to a height of thousands of miles above the true surface of the planet, does certainly seem at first amazing. Yet be it remembered that not only is such an inference demonstrably correct, as we have just shown, but it also follows necessarily from the comparison already instituted between Jupiter and the earth in respect of mass and density. If we assign to the solid globe of Jupiter the same mean density as the earth has—or, rather, if we imagine the totality of material, whence millions of years hence his solid globe is to be formed, gathered into a globe having the same mean density as the earth—we find for this globe a diameter of 53,000 miles, less than his present apparent diameter by nearly 32,000 miles; so that the level of his surface in that condition would lie 16,000 miles below his present surface, the space between the two surfaces, or the total shrinkage of Jupiter's volume, amounting to about 930 times the volume of this earth on which we live. As we have every reason to believe that (in a general sense) all the planets are constructed of the same materials not very

[1] *Cornhill Magazine* for May, 1872.

differently proportioned, we are compelled to admit this vast expansion of Jupiter's present dimensions, and can therefore very well understand even such mighty changes of apparent surface-level as the observation of Admiral Smyth, Sir T. Maclear, and Dr. Peacock certainly shows to have taken place.

But now, reverting to our earth's history during the period corresponding to that through which Jupiter is now passing, let us consider whether the ocean, converted by heat into great cloud-masses floating through hundreds, if not thousands, of miles above the glowing surface-crust, would not produce yet other appearances such as distant observers might have been able to note.

When the shadow of the moon falls now upon the earth during a solar eclipse, it may either wholly or in part reach the actual surface of the earth, or be intercepted partly or wholly by cloud-layers. If an observer on Venus or on Mercury were to watch the earth when undergoing eclipse in this way, the apparent shape of the shadow would not be in any appreciable degree modified by such variations in the manner of the shadow's fall, unless very powerful telescopes were employed. For the cloud-layers of our air lie but a few miles above the surface of the earth,[1] and the apparent

[1] Much less is known than might be respecting the height of the loftier cloud-layers. Coxwell and Glaisher, in their highest aërial flights, saw the cirrus clouds apparently as high above them as when seen from the ground. The height of such clouds could be quite easily determined by taking photographs, with suitably adjusted instruments, from either end of a measured base-line a mile or two in length.

displacement of a part of the moon's shadow, intercepted by a cloud-layer, would be correspondingly small, and in fact undiscernible from Venus or Mercury. But if the atmosphere were very deep, and the cloud-layers separated from each other and from the earth by hundreds of miles, the case would be different. To illustrate the nature of the appearances which might be expected, let us consider the case of a balloon suspended in full sunlight above a layer of fleecy clouds, the layer intercepting a portion of the sun's light, but not all of it. If the layer intercepted all the sun's light, then, of course, a shadow of the balloon would be thrown upon the cloud-layer, this shadow appearing as one, whether seen from the balloon itself, or from the higher parts (let us say) of a lofty mountain reaching far above the layer of clouds. But, the layer not intercepting all the light, a portion of the rays pass on to illuminate the ground everywhere except where the balloon has intercepted the sun's rays. That is to say, there is another shadow on the ground upon the prolongation of lines drawn from the balloon to the shadow on the clouds. These two shadows seen from the balloon itself would appear as one, both lying in the same direction; but they would be separately discernible from a station on the mountain height. Neither would appear quite black; for the higher would lie on clouds through which the observer would receive light from the illuminated ground below, which he would partially see, while the lower shadow would be seen through the illuminated cloud-layer whose light would partially conceal the blackness of the shadow. If the cloud-layer were *very* thin, the upper shadow would be the least distinct; if the clouds without being dense yet suffered but a small quantity of direct sunlight to pass between and through their fleecy texture, the upper shadow would be very dark, the lower scarcely visible. Now replace the balloon by the moon, and the observer upon the mountain height by a distant astronomer on Venus or Mercury, and we perceive that at times, when (in the distant period we are considering) the shadow of the moon fell on a very lofty layer of fleecy clouds, while the shadow so falling would be plainly visible, another fainter one would be discernible on a lower cloud-layer, whose existence and relative position would in this way be indicated to the thoughtful observer. *Or*, if many layers of thin and fleecy clouds, or a single deep layer of such clouds, existed, then either a set of shadows getting fainter and fainter at each successive layer[1] would be seen, or else a long cone of shadow passing through the range of the deep cloud-layer.

Now let us see whether Jupiter, the most conveniently placed of all the younger planets for purposes of observation, shows such appearances as these. Let

[1] The shadows themselves would not grow fainter and fainter, but would be black right through the range along which they would lie; for no part of the sun's rays would reach any one of the spaces in shadow. But seen as they would be through partially transparent cloud-layers, and seen also as the partially illuminated cloud-layers would be *through* the shadows, these necessarily would grow less and less distinct the deeper they lay.

it be premised that *ordinarily* we could not expect to see them, except on very rare occasions, when some exceptionally thin and fleecy cloud-layer, lying very high, received the first shadow, allowing another to be formed on a cloud-layer lying many hundreds of miles below. It would probably be as rare to detect such appearances, supposing them specially searched for (which has never yet happened), as it would be to observe such a phenomenon as the reappearance of a satellite. And manifestly the lower shadow must be hundreds if not thousands of miles below the upper to be separately seen, since the shadow of a satellite would be about 2,000 miles in diameter, and the earth is so close to the sun compared with Jupiter that the line of sight to the planet is never more than slightly inclined to a line from the sun to the planet. Manifestly, if we looked exactly in the same direction as the sun's rays fall, we should not see the shadow at all; looking in a direction slightly inclined, we see the shadow thrown somewhat on one side of the satellite (never *very* far); a lower shadow would be thrown somewhat farther in the same direction, but only (in proportion) very slightly. To be thrown as much as 2,000 miles on one side so as to seem clear of the first shadow, the distance of the lower layer from the upper must be several thousand miles. As for seeing such a cone of shadow as is referred to in the last sentence of the preceding paragraph, that could scarcely ever happen. In fact, if the requisite conditions existed, the chances would be that the lengthened shadow would be too faint to be seen at all. In like manner it might chance that where in reality there was a second shadow it would not be discernible, and the only perceptible effect be that the first shadow would not appear so dark as usual. Probably, on the whole, these being the actual conditions, the reader may consider that it should be all but hopeless to look for any such phenomena as we have referred to, among the recorded observations of the planet.

Let us see how this may be, however. Turning to Webb's little work, 'Celestial Objects for Common Telescopes,' in which we may always expect to find the record of uncommon telescopic observations, we come across the following interesting passage: 'Cassini once failed in finding the shadow of the nearest satellite when it should have been upon the disc. Gorton saw it grey on one occasion. The shadow of the second satellite has been seen specially indistinct by Buffham, Birt, and Grover. South many years ago published in one of the public journals a most interesting observation, which I greatly regret that I cannot recover; but I am confident as to its tenour, which was, that in his great telescope he perceived each of two shadows of satellites on Jupiter to be attended by a faint duplicate by its side, traces of which could be just detected with a smaller telescope of (I believe) five feet,' in focal length. Again, in Chambers's 'Descriptive Astronomy,' it is stated that 'on April 5, 1861, Mr. T. Barneby saw the shadow of the third satellite first in the shape of a broad dark streak such as the cone of the shadow would present in a slanting direction, "but it shortly afterwards appeared as a circular spot, perfectly dark."'

Yet one other observation pointing in the same direction. If the lower shadow of a satellite can be at any time distinguished from the upper, then, should a great cloud-mass be floating at the higher level, its shadow ought to be similarly discernible, projecting to the same extent from under the cloud itself; which would hide the greater portion, but not all, of its own shadow. Now Mr. J. Brett, the eminent landscape painter, who from time to time employs his eye, well cultured to discern varieties of tint, upon the celestial bodies, wrote thus in a paper read before the Astronomical Society in May 1874: 'I wish to call attention to a particular feature of Jupiter's disc, which [the feature, that is] appears to me very well defined at the present time, and seems to afford evidence respecting the physical condition of the planet. The large white patches which occur on and about the equatorial zone and interrupt the continuity of the dark belts are well known to all observers, and the particular point in connection with them to which I beg leave to call attention is that *they cast shadows*; that is to say, the light patches are rounded on the side farthest from the sun by a dark border shaded off softly towards the light, and showing in a distinct manner that the patches are projected or relieved from the body of the planet. The evidence which this observation is calculated to afford refers to the question whether the opaque body of the planet is seen in the dark belts or the bright ones, and points to the conclusion that it is not seen at all in either of them, but that all we see of Jupiter consists of semi-transparent materials. The particular fact from which this inference would be drawn, is, that the dark sides of the suspended or projected masses are not sufficiently hard or sharply defined for shadows falling upon an opaque surface, neither are they sharper upon the light background than upon the dark.' This point Mr. Brett proceeds to deal with by reasoning which has a special value because relating to a subject in which he is an expert. 'The laws of light and shade upon opaque bodies,' he remarks, 'are very simple and very absolute; and one of the most rudimentary of them is that every body has its light, its shade, and its shadow, the relations between which are constant; and that the most conspicuous and persistent edge or limit in this association of elements is the boundary of the shadow; the shadow being radically different from the shade in that its intensity is uniform throughout in any given instance, and is not affected by the form of the surface on which it is cast, whereas the shade is distinguished by attributes of an opposite character. Now if the dark spaces adjoining the light patches on Jupiter, which I have called shadows, are not shadows at all, but shades, it is obvious that the opaque surface of the planet on which the shadows should fall is concealed; whereas, if they are shadows, their boundaries are so soft and undefined as to lead to the conclusion that they are cast upon a semi-transparent body, which allows the shadow to be seen, indeed, but with diminishing distinctness towards its edge, according to the acuteness of its angle of incidence. Either explanation of the phenomenon may be the true one; but they both lead to the same conclusion—namely, that neither the dark belts nor the bright ones are opaque, and that if Jupiter has any nucleus at all, it is not visible to us. . . . By the kind invitation of Mr. Lassell I had an opportunity, on the 20th of April, of examining the disc with his twenty-feet reflector of two-feet aperture, and I found this large instrument confirm my impressions concerning the shadows in the most satisfactory manner.'

There remains one peculiarity in the appearances resulting from the earth's condition during the remote period we are dealing with, which might possibly, though perhaps *barely*, have been detected by observers on Venus or Mercury. The shadow cast by the earth upon the moon—that is, the true shadow, not the mere penumbra—has a round shape, corresponding to the fact that the body casting it is a globe. But of old, when irregular cloud-masses and cloud-layers, various in shape and extent were suspended in the deep atmosphere of our planet, it must necessarily have happened that at times the outline of the shadow was irregular, and that in a marked degree. The irregularity, in fact, would correspond closely in degree with the occasional irregularity of the earth's apparent figure arising from the same cause (though it is possible that it might have been at times more clearly discernible, as not affected to quite the same degree by irradiation). Now here is a peculiarity which we could not expect to recognise in the case of our heretofore chief test-planet, Jupiter. No telescope yet made by man, probably no telescope man ever will make, would show peculiarities in the shape of Jupiter's shadow on one of his satellites. No one has ever yet claimed to have seen the outline of that shadow at all, far less to have been able to discern its true shape; and it is not likely that anyone ever will. But in this case the planet Saturn may help us; for *his* shadow is not merely cast at times upon the small discs of his distant moons, but rests constantly upon the broad expanse of his mighty rings—

> While Saturn whirls, his steadfast shade
> Sleeps on his luminous ring—

and that shadow we can study, despite the vast distance of the planet, with a fair chance of detecting peculiarities in its shape, should such at any time exist.

Let it be noticed at the outset that it is perfectly easy to calculate what the shape of the shadow *should* be, if Saturn were a solid globe and the rings' surface perfectly flat. The astronomer knows that at one time, on these assumptions, the shadow would be hidden, at another visible above or below the planet's globe; at one time to the east of the globe, at another to the west, and always with an elliptical (but very nearly circular) outline, not quite sharply defined, but with a slight fringe of shading only discernible in powerful telescopes. In like manner we may note, in passing, that the shape of the rings' shadow on the globe would always be calculable; and we know that, when visible at all, it should appear as a black curved streak, either above or below the ring, and perfectly smooth in outline. Again, whatever irregularities there may be in the level of the rings can very little affect the apparent shape of either shadow, because we know from the edge view of the rings that such irregularities are slight compared with the thickness of the rings, which itself is not great. So that any irregularities of a marked character in either shadow must be referred to that cause alone which is competent to produce them; viz. irregularity in the cloud-layers and cloud-masses floating in the deep atmosphere of the planet.

So much premised, let us see what the records gathered by astronomers have to tell us on this point. We turn to a series of papers on the planet Saturn in the 'Intellectual Observer' for 1866, by Mr. Webb, and we find the portion relating to the shadows opening thus: 'From an early period, irregularities have been remarked in the form of the shadows which the globe and ring mutually cast upon each other.' Mr. Webb deals first with the shadow of the ring, with which at present we are not directly concerned; though, of course, any irregularities in that shadow, like the irregularities in the shadows of Jupiter's moons, already described, indicate the depth and the occasionally irregular arrangement of the cloud-envelopes. Mr. Webb, in fact, after describing such irregularities, rejects, first, the theory that they are caused by irregularities in the ring; secondly, the theory that the globe's surface is irregular; and, thirdly, the theory that the ring has an atmosphere through which the sun's rays are irregularly refracted—in fine, 'passing over this difficulty as insoluble,' which is not a very satisfactory result. Going on to consider the shape of the shadow of the planet on the rings, he mentions, first, how such first-rate observers as Sir W. Herschel, Lassell, Dawes, and Secchi saw the outline of the shadow concave, instead of convex. Next, Dawes on one occasion saw the shadow irregular in outline where it crossed the bright ring. In October, 1852, Lassell saw the shadow on *both* sides of the globe. The younger Bond, of Harvard, Mass., saw the same; on November 2, saw the shadow *winged.* November 3, Tuttle saw the shadow on both sides, on which he naïvely asks: 'What can this mean?' On November 29, De la Rue saw the shadow on both sides, and wrote: 'This is very remarkable, but there can be no question as to the fact;' both shadows looked 'like objects seen by mirage'—a remarkable expression. Then we find these observers, and others of equal repute, describing the shadow as having horns, ears, a 'roof' (pictured with two projecting eaves), an inlet, a single ear, a reversed edge. Secchi writes: 'L'ombre assez curieuse, elle est renversée et ondulée.' On one occasion Bond saw two shadows—one black, the other 'a narrow, ghost-like shade.' Of this faint shadow he says: 'I was much impressed by the fact that the outline was preserved perfectly, while the intensity of the shadow was very feeble.' Was not this *certainly* either the faint shadow of a deep partially transparent cloud-layer, or a dark shadow seen *through* such a layer?

After enumerating a number of such cases, Mr. Webb proceeds: 'Thus far extend our facts. What shall we say in explanation of them? Can we charge them upon personal or instrumental peculiarities?[1] It seems not possible, since, in the main, they are agreed upon in England and Italy, and Malta, and India, and the United States. Some of the most singular statements, it is true, come from America alone. But, as they have often the concurrence of more than one observer, so the optical capacity of a telescope, which in favourable air would bear distinctly a power stated to be 1,560, leaves small chance of appeal.' (He might have added that the American astronomers were second to none in observing skill, and that the American skies are particularly favourable for observations of the class in question.) 'In fact, it is,' Mr. Webb proceeds, 'a remarkable circumstance that the mystery of the subject has increased under closer, more powerful, and more extended scrutiny. Some of the phenomena may admit of a more or less probable solution. For instance, the apparent concavity of outline might be explained as a deception similar to those optical perversities illustrated by Mr. Proctor,' in an article on Saturn's square-shouldered aspect. 'But the "ears" projecting, even when the true shadow was invisible—

[1] We have altered a word here, and perhaps marred the sentence; but the original word 'equation' would have no meaning for many readers of these pages.

the two shadows, when one only should have been seen—the "roof" and "inlet," and the varying depths of shade in different parts, are alike too clearly attested for doubt, and too incomprehensible for explanation.' (*Cela depend.*) 'We might take refuge to a certain extent in the idea of varied curvatures in the shadowed surfaces; and, in order to meet the objection arising from the evanescent thinness of the rings,' we might 'speculate on some force emanating from the sun disturbing the level of the rings. But even after we have ventured this daring' (and, in fact, impossible) 'effort, we find other features as intractable as ever. Some things look like effects of an atmosphere very irregularly distributed round the ball, and possessed of properties greatly dissimilar to those of ordinary gases; but this is undiscoverable, just where it ought to be most apparent,' where the remoter parts of the ring meet the outline of the disc obliquely.

But there is not one of these phenomena which cannot be explained by the theory of a very deep atmosphere, not 'irregularly distributed,' or 'possessing properties greatly dissimilar to those of ordinary gases,' but irregularly laden with cloud-masses. In fact, these occasional peculiarities in the shadow are thus brought into exact correlation with the peculiarities observed occasionally in the planet's shape, as noted in the first part of this paper.

We might note here other circumstances in the earth's youthful condition. For instance, from time to time the ruddy glow of her intensely heated surface must have been visible through breaks in her cloud-layers; and just such occasional views of Jupiter's heated surface seem to have been obtained on those occasions when the usually cream-white equatorial belt has shone with a ruddy colour. But this consideration, and others connected with the quantity of light received from Jupiter and Saturn, have already been dealt with at considerable length in these pages.

It appears to us, in fine, that all the evidence, both *à priori* and *à posteriori*, corresponds with the theory which we have brought before the reader, that a planet during its extreme youth has its oceans floating in the form of cloud-masses and cloud-layers in a very deep atmosphere. We have seen reason, first, for believing that the intense heat of a planet, for many ages after its first formation, would keep the oceans in this cloud-like condition. Then, looking around for planets such as we might suppose to be much younger than the earth, we have seen that Jupiter and Saturn, the giant planets of the solar system, are probably the youngest (in this sense), always excepting the sun, which is in an earlier stage than any member of his family. And, considering what appearances a planet with a very deep cloud-laden atmosphere might be expected to present, we have found that just such appearances are presented by the planets Jupiter and Saturn, the phenomena described not being seen at all times, but occasionally, and in varying degree, precisely as we should expect from the variable causes producing them. We have also seen that the small density of the giant planets cannot readily be otherwise explained than by the theory that we do not see their real surface, but the outer surface of cloud-layers enveloping them. Moreover, while not a single fact known about the great planets is opposed to this theory, there are some facts, as we have seen, which cannot *possibly* be explained on any other theory. But when so much as this can be said of any theory, the theory may be regarded as established.

When the earth and sea were young, then, the earth's whole frame was intensely heated. Her real surface was doubtless partly solid and partly liquid then, as now; but the solid portion glowed with ruddy and in places with white heat, while the liquid portions, instead of being water, as now, were formed of molten rock. Above this surface, with its 'tracts of fluent heat,' was the fiery atmosphere of that primeval time, enormously deep, complex in constitution, bearing enormous masses of aqueous vapour, and every form of cloud and cloud-layer, swept by mighty hurricanes whose breath was flame, drenched with showers so heavy that they might rather be called floods, and tortured by the uprush of the vaporous masses formed as these floods fell hissing on the earth's fiery surface.

After myriads of centuries came the time when the surface so far cooled as no longer to glow with ruddy light, and no longer to reject by vaporising the waters which fell upon it. Then a fearful darkness prevailed beneath the still mighty canopy of cloud; for only little by little, by very slow degrees, would the water descend upon the earth's surface. Some, indeed, have thought that it was this stage of the earth's past which was described in the Bible words: 'The earth was without form and void, and darkness was upon the face of the deep;' noting, in particular, that the coming of light (because of the descent of the waters upon the earth, according to this view) was followed by the separation of waters under the firmament from waters above the firmament, (that

> Expanse of liquid, pure,
> Transparent, elemental air,)

the waters under the heaven being next gathered together into one place, and so forth. But we must confess that this interpretation of the narrative, sometimes called the vision interpretation, seems to us very far-fetched and unnatural; though we are in no way concerned here to oppose it, deeming it only necessary to mention that, for our own part, we cannot doubt that the writer of the narrative wished to be understood as describing what really occurred, not appearances shown to him in a vision.

A question which has long been regarded as among the great mysteries of nature—the question, How did the seas become salt?—seems to us to find a ready solution when we consider that the ocean once formed the earth's cloud-envelope. We may, in fact, regard the oceans as holding in solution what was washed from the earth or otherwise extracted from its substance during the ages when the waters of ocean were passing from their former to their present condition. For then all the conditions assisted the action of the waters themselves—the intense heat of the earth's crust and of the atmosphere, the tremendous atmospheric pressure, and consequently the high boiling point (so that the waters first formed on the earth's heated crust must have been far hotter than is boiling water at the present time), and the presence also in the atmosphere of many vapours which would greatly help the decomposing action of the water itself. Consider, for instance, the following description, abridged from a paper by Dr. Sterry Hunt, the eminent Canadian chemist and geologist. After showing that carbonic acid, chlorine, and sulphurous acids would be present in enormous quantities in the primeval atmosphere, besides, of course, still vaster quantities of the vapour of water, he proceeds: 'These gases, with nitrogen and an excess of oxygen, would form an atmosphere of great density. In such an atmosphere, condensation would only take place at a temperature far above the present boiling point; and the lower levels of the earth's slowly cooling crust would be drenched with a heated solution of hydrochloric acid, whose decomposing action, aided by its high temperature, would be exceedingly rapid. The primitive igneous rock on which these showers fell probably resembled in composition certain furnace slags or volcanic glasses.' The process of decomposition would continue 'under the action of the heavy showers until the affinities of the hydrochloric acid were satisfied. Later larger quantities of sulphuric acid would be formed, and drenching showers of heated solutions of this energetic dissolvent would fall upon the earth's heated crust. After the compounds of sulphur and chlorine had been separated from the air, carbonic acid would still continue to be an important constituent of the atmosphere. It would be gradually diminished in gravity,' through chemical processes resulting in the formation of various clays, 'while the separated lime, magnesia, and alkalies, changed into bicarbonates, would be carried down to the sea in a state of solution.'

Here we seem to see a fair account rendered of the enormous quantity of matter forming collectively what is called the brine of the ocean, and containing, besides common salt (chloride of sodium), sulphuric acid, magnesia, soda, sulphate of lime, and other substances. The theory that these substances have been washed from the earth's surface by causes such as are now in progress, would not, we think, be seriously entertained if the vast amount of matter thus present in the waters of the sea were remembered and considered. Brine forms, on the average, about 3½ per cent. of sea-water. Hence, if we take the average depth of the ocean at two miles,[1] or, roundly, 10,000

[1] In Maury's *Physical Geography of the Sea* there is a passage which we take to be one of the most amusing ever written in a work of the kind. The idea would seem to have occurred to him of estimating how much surface the salts of the sea would cover to the depth of a mile; and while in the midst of the calculation, he would seem to have grown weary of it. At least we cannot otherwise understand how he came to pen the following singular remarks: 'Did anyone who maintains that the salts of the sea

feet, it follows that, if all the water of the sea were evaporated, there would be left a deposit of salt averaging 350 feet in depth all over the present floor of

were originally washed down into it by the rivers and the rains ever take the trouble to compute the quantity of solid matter that the sea holds in solution as salts? Taking the average depth of the ocean at three miles, and its average saltness at $3\frac{1}{2}$ per cent., it appears that there is salt enough in the sea to cover to the thickness of one mile an area of *several millions of square miles.*' (The italics are ours.) This passage reminds us of one in an early volume of *Household Words*, where a very amusing account was given of the stores of wine in the London Docks, over which the writer is supposed to be shown, collecting materials, *but also tasting wine*, as he proceeds. The gradually increasing effect of the wine-tasting is indicated very humorously. In one of the later stages of his progress, the writer enters into a computation of the amount of wine wasted in the process of cleansing the glass with wine. (We write from memory, and possibly, as many years have passed since we read the passage, we may not be correct in details.) Assuming so much wasted at each cleansing, so many visitors, each tasting so many times, and so forth, 'then,' says the writer, 'it may be shown that in each year 800 bottles, or it may be 8,000 bottles, of wine are wasted. And should any one object that there is a considerable difference between 800 and 8,000, all we have to say is that the principle is the same,' &c. Captain Maury passes on, however, without any allusion to the somewhat unexpected vagueness of his conclusion. 'These millions of cubic miles of crystal salt have not made the sea any fuller,' he proceeds. 'All that solid matter has been received into the interstices of sea-water without swelling the mass; for chemists tell us that water is not increased in volume by the salt it dissolves. Here we have, therefore, an economy of space calculated to surprise even the learned author himself of the *Plurality of Worlds.*' All which, so far as appears, is *a propos de bottes.* Within the same page, which, we submit, is inferior to Maury's usual style, we find him, in dealing with the question, What was the Creator's main object in making the sea salt? advancing the startling proposition that 'all the objects of the salts of the sea are *main* objects.' (The nature of the context, which is serious, even solemn, will not allow us to suppose that any pun was here intended.)

the sea. This would correspond in quantity to salt covering all the present land surface of the earth to a depth of a thousand feet, or to a deposit *two hundred feet deep over the entire surface of the globe*; so that the idea of its having been washed from the land is altogether inadmissible. It may, indeed, be urged that, as the process of washing down from the land is continually going on, only a sufficiency of time would be needed to account for any quantity whatever of sea-salt. But apart from the fact that only a certain thickness of the solid crust, and that thickness by no means very great, could be drawn upon for the supply, and that the very continuance of the process shows us that even that portion of the earth's crust has not been drained of its salts, there is every reason to believe that the extraction of salt from the sea is going on and has been going on for many ages past at fully as great a rate as the addition of fresh salts. Although the process of evaporation cannot remove the salts, these, as Maury justly notes, can be extracted by other processes. 'We know,' he says, 'that the insects of the sea do take out a portion of them, and that the salt-ponds and arms which from time to time in the geological calendar have been separated from the sea, afford an escape by which the quantity of chloride of sodium in its waters—the most abundant of its solid ingredients—is regulated. The insects of the sea cannot build their structures of this salt, for it would dissolve again as fast as they could separate it. But here the ever-ready atmosphere comes into play, and assists the insects in regulating the salts. It cannot take them [the salts] up from the sea, it is true, but it can take the sea away from them; for it pumps up the water from these pools that have been barred off, transfers it to the clouds, and they deliver it back to the sea as fresh water, leaving behind the salts it contained in a solid state. These are operations which have been going on for ages; proof that they are still going on is continually before our eyes; for the 'hard water' of our fountains, the marl-banks of the valleys, the salt-beds of the plains, Albion's chalky cliffs, and the coral islands of the sea are monuments in attestation.'

We must, then, regard the salts of the sea as in the main dissolved from the solid crust during that remote period when the seas were young. The seas thus indicate to us the nature of those vast chemical processes through which the earth had to pass in the earlier stages of its history. If the present crust of the earth did not afford, as it does, the clearest evidence of a time when the earth's whole frame glowed with intense heat; if we could not, as we can, derive from the movements of the celestial bodies, as well as from the telescopic appearance of some among them, the most certain assurance that all the planets, nay, the whole of the solar system itself, were once in the state of glowing vapour; the ocean brine—the mighty residuum, left after the earth had passed through its baptism of liquid fire, would leave us in little doubt respecting the main features at least of the earth's past history. The seas could never have attained their present condition had not the earth which they encompassed when they were young been then an orb of fire. Every wave that pours in upon the shore speaks to us of so remote a past that all ordinary time-measures fail us in the attempt to indicate the length of the vast intervals separating us from it. The saltness of the ocean is no minor feature or mere detail of our globe's economy, but has a significance truly cosmical in its importance. Tremendous indeed must have been the activity of those primeval processes, fierce the heat of those primeval fires, under whose action sixty thousand millions of millions of tons of salts were extracted from the earth's substance and added to its liquid envelope.

[Since this essay was in type, a paper has been read before the Astronomical Society by Mr. Brett, describing observations altogether inexplicable, except by the theory we have advocated above. They relate to the movements of two large white spots on Jupiter's chief belt. Both these spots were so shadowed as to indicate that they were in reality bodies of globular shape—no doubt rounded masses of cloud, floating in the relatively transparent atmosphere of the planet. 'The fact that they are wholly immersed in the semi-transparent material of the planet is indisputable,' says Mr. Brett, 'since they gradually disappear as they approach the' edge of the disc, 'and in no case have been seen to project beyond it.' The distinguishing peculiarity of these bodies was, however, their rapid motion, as though gaining on the planet's rotation. The average motion was estimated by Mr. Brett at about 165 miles per hour, but this estimate would have been somewhat reduced had he taken into account, as he should have done, the changing position of the earth, relatively to Jupiter. Still, even after adding to this reduction all that can possibly be attributed to errors of observation, there remains a considerable motion of these cloud-masses, each of which was about half as large as the whole

IS THE MOON DEAD?

THE idea generally prevailing, among astronomers, respecting the moon's condition is that she is a dead planet, an orb which circles around the sun like her companion planet the earth, but is not, like the earth, the abode of living creatures of any sort. Formerly, indeed, other views were entertained. It was thought that the dark regions were seas, the bright regions continents—a view embodied by Kepler in the saying, 'Do maculas esse maria, do lucidas esse terras.' But the telescope soon satisfied astronomers that there are no seas upon the moon. It has been noted that in two well-known passages of the 'Paradise Lost,' in which Milton touches on the work of Galileo with the telescope, he speaks of lands, mountains, rivers, and regions, but not of oceans or seas, upon the moon. Thus, in describing the shield of Satan, he compares it to

> The moon, whose orb
> Through optic glass the Tuscan artist views
> At evening from the top of Fesolé,
> Or in Val d'Arno, to descry new lands,
> Rivers, or mountains, on her spotty globe.

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globe of the earth! It may, perhaps, be thought that we have here attached too much weight to the telescopic observations of one who is skilled rather in art than in science; and in fairness it must be admitted that about half Mr. Brett's observations have been regarded more than doubtfully by astronomers. But this observation, like the one described in the body of the above essay, depends only on accuracy in estimating the apparent position of two spots on the planet's face; and so skilful a draughtsman as Mr. Brett cannot have made any large error in an observation of the kind.]

While again, in the fifth book, Raphael views the earth

> As when by night the glass
> Of Galileo, less assured, observes
> Imagined lands and regions in the moon.

We may well believe that had Galileo, in his interviews[1] with Milton, described appearances which (with his telescopic power) resembled seas or oceans, the poet would not have used so vague a word as 'regions' in the third line of the last quoted passage, where the word 'oceans' would so obviously have suggested itself. From the very beginning of the telescopic observation of our satellite, it became clear that no seas or oceans exist upon her surface. And as telescopic power has increased, and the minute details of the moon's surface have been more searchingly scrutinised, it has been seen that there are no smaller water regions, no lakes, or rivers, not even any ponds, or rivulets, or brooks.

But indeed, while the close telescopic scrutiny of the moon was thus showing that there are no water surfaces there, it was becoming also clear that no water could remain there under the sun's rays; that is, on the parts of the moon which are illuminated. For it was found that the moon has an atmosphere so rare that water would boil away at a very low temperature indeed. How rare the lunar atmosphere is we do not certainly know; but a number of phenomena show that it must be very rare indeed. Some of these have been already considered, along with other lunar pheno-

[1] See Milton's *Areopagitica.*

mena, in an article which appeared in the 'Cornhill Magazine' for August 1873; and for this reason (especially as that article has since been republished) we do not here enter into this portion of the evidence, our object being to discuss here certain relations which were not dealt with in that earlier paper.

But now that astronomers have almost by unanimous consent accepted the doctrine of the development of our system, which involves the belief that the whole mass of each member of the system was formerly gaseous with intensity of heat, they can no longer doubt that the moon once had seas and an atmosphere of considerable density. The moon has, in fact, passed through the same changes as our own earth, though not necessarily in the same exact way. She was once vaporous, as was our earth, though not at the same time nor for so long a time. She was once glowing with intensity of heat, though this stage also must have continued for a much shorter time than the corresponding stage of our earth's history. Must we not conclude that after passing through that stage the moon was for a time a habitable world as our earth is now? The great masses of vapour and of cloud which had girt our moon's whole globe, even as in the youth of our earth her seas enwrapped her in cloud form, must at length have taken their place as seas upon her surface. The atmosphere which had supported those waters must at first have been dense by comparison with the present lunar atmosphere, perhaps even by comparison with the present atmosphere of our earth. Then the glowing surface of the moon gradually cooled, until at length the moon must have been a fit abode for life. But whether, when thus swept and garnished into fitness for habitation, the moon actually became an inhabited world, is a question which will be variously answered according to our views respecting the economy of nature in this respect. Those who hold that nature makes nothing in vain, will need only to ask whether the support of life is the one sole purpose which a planet can subserve; if that should appear probable, they would at once decide that the moon must during its habitable stage have been inhabited. Others who, looking around at the workings of nature as known to us, perceive, or think they perceive, that there is much which resembles waste in nature, will be less confident on this point. They may reason that as of many seeds which fall upon the ground, scarce one subserves the one purpose for which seeds can be supposed to have been primarily intended, as many younglings among animals perish untimely, as even many races and types fail of their apparent primary purpose, so our moon, and possibly many such worlds, may never have subserved and never come to subserve that one chief purpose for which the orbs peopling space can be supposed to have been formed, if purpose indeed reigns throughout the universe.

But we are not here concerned to inquire carefully whether the moon ever was inhabited; we care only to show the probability, the all but certainty, that the moon during one stage of her existence was a habitable body, leaving the questions whether she ever actually had inhabitants, and what (if she had) their nature may have been, to the imagination of the reader. Most certainly there is little reason for believing that on *this* point men will ever have any real information for their guidance.

But it will be well, in thus considering the past of our moon for the purpose of forming clearer conceptions as to her present condition than telescopic evidence can supply, to examine not only in what respects she resembled our earth, but also those circumstances in which there is reason to believe that she differed.

In the first place, whatever opinion we form as to the exact nature of the processes by which the solar system attained its present stage of development, we cannot doubt that our moon was formed earlier than the earth. In considering the solar system we may be doubtful whether an exterior planet was fashioned before or after an interior planet, a larger planet before or after a smaller planet, and so on; but we could not for a moment doubt, even if the sun's present aspect did not assure us of the fact, that the fashioning process was completed earlier for each one of the planets than for the central body. In thus speaking, it will be understood that we are referring to the stage of cosmical existence when the great bulk of a planet's material has been gathered in, and the planet has assumed the condition of a separate globe of matter. So long as any considerable portion of the materials out of which a planet was to be formed, continued so placed and so moving as to be still associated with the sun's exterior portions, neither the sun nor the planet could be regarded as having yet assumed their final form; and it is manifest that, when planet after planet had taken its substance from the general stock of matter, the sun must still have had long processes of change to undergo before he could gather in his substance in such sort as to become a definite orb, separated by a well-marked interval from the nearest member of his system. In the same sense it is clear that long after our moon had been formed as a separate body circling round the centre of gravity of the mass from which she herself had been framed and the earth was hereafter to be framed, the earth must have been but an inchoate planet, a chaotic mass of vaporous matter, without even as yet, in all probability, a solid or liquid nucleus.

Thus started in planetary life long before the earth, the moon must also have possessed far less heat when thus first formed than the earth when formed long after. The inherent heat of a planet in the first stage of its existence, must depend on the quantity of matter it contains, whichever theory of development we adopt. If a planet were solely formed, as Laplace's theory supposes, from nebulous matter left behind after the contraction of the great nebulous mass of the system, the heat of the planet, when its vaporous mass had so contracted as to give the planet separate existence, would depend on the quantity of vapour; seeing that in every part of the process of contraction heat would be evolved in proportion to the quantity of contracting vapour and the energy of the contractile force, this energy being itself proportioned to the mass or quantity of vapour. If, on the other hand, a planet were formed solely by gathering in cometic and meteoric matter, then the planet's original heat would depend on the energy with which such matter was drawn in; and this energy would greatly depend on the quantity of matter gained by the planet. Assuming, as far more probable than either theory, that planets were formed by a combination of both processes, of course what is true of each process considered separately is true of the two combined. Applying this reasoning to the moon, whose mass is but about one eighty-first part of the earth's, we see that when she was first formed, her heat, though doubtless intense, was far less than the heat of the earth at the corresponding stage of planetary life. Here, then, was a second cause tending to shorten the duration of the moon's life as compared with that of our earth, or rather tending to throw back, and that not by a little but by millions of years, the habitable period of the moon's existence.

But, thirdly, the moon must have passed much more rapidly through all the stages of planetary life than our earth. We have already shown ('Cornhill Magazine' for August, pp. 157, 158) that the rate of a planet's cooling depends on the quantity of matter contained by the planet. If we applied the same method to the moon which we there applied to Jupiter —with the same caution, however, as to any exact reliance to be placed upon the result—we should infer that each stage of the moon's existence would fall short by many millions of years of the corresponding stage of the earth's. Thus, the experiments of Bischoff, there referred to, indicate some 350 millions of years as the period required for the cooling of the earth from 2,000° centigrade to the heat of boiling water. Now the experiment on heated iron globes, also described there, would show that, as the moon's mass is but about the eighty-first part of the earth's, the moon would require but about three thirteenth parts [1] of this period, or about eighty millions of years, to pass through the same stage of cooling. Here, then, there is a difference of no less than 270 millions of years for one stage only of the moon's cooling. And although, as we pointed out in the essay to which we have just referred, exact reliance cannot be placed upon the method of calculation here employed, yet the real difference is as likely to exceed as to fall short of the result just indicated. It is certain, or as nearly certain as anything not actually demonstrated can be, that the difference must be measured by tens of millions of years.

There is yet one other circumstance which must have tended to shorten the moon's existence as a habitable world. We know that the earth's vulcanian forces, by which her frame is modelled and remodelled so as to be continually refitted for the purposes of life, depend on the energy of terrestrial gravitation. This we shall presently have to consider more at length; at present

[1] It will be found that $4\frac{1}{3}$ multiplied twice into itself gives a number not much greater than eighty-one.

we need merely note that it is terrestrial gravitation, drawing continually inwards the rock strata of our earth's frame, which results (on the conversion of the mechanical energies thus arising, into heat) in the generation of the pressures on which earthquakes, volcanoes, and other such phenomena depend. The relative smallness of lunar gravity must have caused these processes to continue for a much shorter time in her case than in our earth's.

Combining together these considerations—viz. first that the moon must have been fashioned as a planet many millions of years before the earth; secondly, that her original heat must have been greatly less than that of the earth (corresponding to a reduction of many millions of years in the time required for cooling down to the habitable condition); thirdly, that each stage of the moon's cooling must have lasted less by many millions of years than the corresponding stage for the earth's cooling; and lastly, that lunar gravity being so much less than terrestrial gravity the moon's vulcanian vitality must have lasted for a much shorter time than the earth's—we perceive that the moon must have passed that stage of her history which corresponded to that through which our earth is now passing, many many millions of years ago. It would probably be no exaggeration whatever of the truth to say that more than a thousand millions of years have passed since the moon was a habitable world. But we may quite confidently assert that fully a hundred millions of years have passed since that era of her history. And as the changes which she has undergone since then have occurred at a much more rapid rate than those by which the earth is now passing on and will continue to pass on, for ages yet to come, towards planetary decrepitude, we may assert with equal confidence that the moon is passing through a stage of planetary existence which the earth will not reach for many hundreds of millions of years yet to come. The moon, thus regarded, presents to us a most interesting subject of study, because she illustrates, in general respects if not perhaps in details, the condition which our earth will attain in the remote future.

Let us then examine the principal features of the moon—those which may be regarded as characteristic, which at any rate distinguish her from the earth—and consider how far it is probable that our earth will one day present similar features. We can also inquire how far the moon's present condition may be regarded as that of a dead world, in this sense that she can neither now be, nor (under any conceivable circumstances) hereafter become, once again a habitable world, as formerly she presumably was.

There is one very remarkable feature of the moon's motions which is commonly not explained as we are about to explain it, but in a way which would correspond better with the general views indicated in this article, than the interpretation which seems to us preferable. We refer to the circumstance that the moon's rotation on her axis takes place in precisely the same time as her revolution around the earth. This is, in reality, a very strange feature, though it is often dismissed as if there were nothing very remarkable about it. In whatever way the arrangement was brought about, it is absolutely certain that the earth had her share in the work; and again, no matter what explanation or set of explanations we accept, we find most interesting evidence suggested as to the moon's past condition.

According to one account, the moon was originally set spinning at a rate closely corresponding to her present rotation rate, and the earth, having by her attractive power somewhat elongated the moon towards herself, acted on this not perfectly round body in such sort as gradually to coerce its motion of rotation into exact agreement with its motion of revolution. It is known that this would necessarily happen if the original approach to agreement between these motions had been sufficiently close. If we adopted this view, we should find ourselves in presence of the somewhat remarkable fact that the small moon was in the beginning set rotating so slowly that its day lasted as long as a lunar month. Such a rotation, as the result of some process of systematic evolution, could be readily accepted; but that this motion, which presents no recognisable advantages, and many most manifest inconveniences (for creatures living in the moon), should have been specially communicated to the moon by the creative hand, would not be an acceptable theory, even if we were not forced by overwhelming evidence to throw special creative acts very much farther back (to say the least) than the formation of our moon, or of any part of the solar system.

Another explanation which has been offered runs as follows. When the moon had oceans, the earth must have acted on those oceans in the same way as the moon now acts on the oceans of our earth. In one respect the earth must have acted more energetically, in another less. Being very much (eighty-one times) more massive than the moon, the earth necessarily exerts much more force on the moon's substance than the moon exerts on hers.[1] On the other hand, the relative *difference* between the pull on the nearest and remotest parts of the globe is less in the case of the earth drawing the waters of the moon (in old times) than in the case of the moon drawing the waters of the earth; for the moon is a much smaller globe than the earth; and this difference is the really effective force in the production of tides. Also it is probable that the moon never had a relatively large ocean-surface, as will presently be shown, and small seas (probably disconnected) could not be swept by a great tide-wave. Still we may suppose that there was once a tidal wave, greater or less, sweeping athwart the lunar seas much in the manner of our own tidal wave. Now, our tidal wave is beyond doubt slowly checking the earth's motion of rotation, for the wave travels so as to meet

[1] In one sense the moon pulls the earth just as strongly as the earth pulls the moon, for gravity is not a force which one body exerts on another solely, but a mutual force. But what mathematicians call the moving force exerted by the earth on the moon is eighty-one times greater than the corresponding force exerted by the moon on the earth; for the mutual attraction between these bodies has in the former case to move the moon, whereas in the latter it has to move the much larger mass of the earth.

the motion of rotation, which therefore to some slight degree it opposes. This will go on, until at length the rotation has been so reduced that the tidal wave no longer affects it; or, in other words, until the earth's period of rotation corresponds with the period of the tidal wave, viz. with the lunar month. Hundreds of millions of years will pass before that happens; but then we have seen that the moon *may* fairly be regarded as illustrating the earth's condition hundreds of millions of years hence. Accordingly, there is nothing absolutely incredible in the theory that during the remote ages when the moon had seas the tidal wave which traversed them, continually retarding the moon's motion of rotation, gradually coerced it into absolute agreement with her motion of revolution around the earth. Still it must be admitted that the theory is not very easily to be accepted as it stands. The seas of the moon were probably less in relative extent, even when at their largest, than those of Mars now are, and such seas could have no tidal waves which even in thousands of millions of years could reduce the moon's rate of rotation in any considerable degree; and, as we shall presently see, the duration of the era when the moon had seas can hardly have been measured by periods so vast. On the whole, while we may admit the probability that at some very distant time in the past the earth may have exerted influences on lunar seas resembling those which the moon now exerts on our seas, it does not appear to us probable that the peculiar feature we are now considering can be attributed either wholly or in very large degree to the retarding influence of tidal waves upon the moon.

One other theory remains which seems to have more in its favour than either of those hitherto considered. Before the moon became a separate planet her frame, then vaporous, must have been enwrapped in the vaporous frame of the earth. While this continued the moon was necessarily compelled to move as a portion of the earth's outer envelope, and therefore, of course, turned upon her axis in the same time that that exterior portion of the earth revolved. So soon as the contraction of the earth's vaporous frame left the moon outside, she was free *if she could* to change her rate of rotation; that is to say, the earth's enwrapping vapour-masses no longer prevented the moon from changing her rotation rate. And there were two causes at work, either of which, if in action alone, would have markedly changed the moon's rate of turning on her axis. *One* was the gradual contraction of the moon's frame in cooling. This would have made her turn more quickly on her axis. *The other* was the continually gathering in of meteoric matter from without, which was a process taking place probably far more rapidly then than now, seeing that the meteoric systems now remaining are the merest residue of a residue compared with those existing hundreds of millions of years ago. This process would tend to make the moon turn more slowly upon her axis. However, the former process would probably operate far more effectively, and thus the moon would on the whole have acquired a more rapid rate of rotation, and the coincidence between rotation and revolution existing when she first had separate existence would have disappeared. But there was all the time a force at work to check the tendency to change in this respect. The earth was there, exerting that very force which we have already described in considering another theory—a force competent, we may infer, to check the tendency to a slow increase in the moon's rate of rotation, and to preserve that relation which existed when the moon was first formed. We say that the competence of this force may be inferred—meaning that the observed coincidence between the moon's rate of turning round upon her axis, and her rate of revolution around the earth, shows that the force was sufficient for that purpose. A similar force exerted by the sun upon the earth since she was first separately formed has not proved competent, as we know, to make the earth turn on her axis in the same time exactly that she travels round the sun—that is, in a year. Nor have any of the planets been forced to behave in this way. But we can readily understand that a great difference should exist between the formation of a planet which, having an enormously high temperature when first formed, would have an enormous amount of contraction to undergo; and the formation of a subordinate orb like the moon, which, though no doubt intensely hot when first thrown off[1] by the contracting earth, cannot have

[1] We here use the words 'thrown off' as equivalent to 'left behind.' The theory that the moon was thrown off by the earth, or

been nearly so hot as a planet at the corresponding stage of its existence. On the whole, there are (so it seems to us) good reasons for believing that that peculiar law of the moon's motion which causes the same lunar hemisphere to be constantly turned earthwards had its origin during the birth itself of our satellite. We may, indeed, find in that peculiarity one of the strongest arguments in favour of the theory that our solar system reached its present condition by a process of development, since on no other theory can a satisfactory solution be obtained of the most striking peculiarity of the moon's motions.

But the inhabitants of earth are more directly interested—not for their own sake, but for the sake of their remote descendants—in the subject of the moon's present airless and waterless condition, regarded as the result of systematic processes of change. If we can ascertain what those processes may have been, and if we should find that similar processes are taking place, however slowly, on the earth, then the moon's present condition has in a sense the same sort of interest for us that a man in the full vigour of life might be supposed to find in the study of the condition of aged persons, if through some strange chance he had never had an opportunity of observing earlier the effects of old age upon the human frame. The inhabitant of earth who contemplates the moon's present wretched condition, may be disposed—like Lydia Van den

the earth by the sun, is altogether inconsistent with mechanical possibilities.

Bosch when she saw Madame Bernstein's shaky hands and hobbling gait—to hope we 'sha'n't be like her when we're old, anyhow;' but the probabilities are in favour of a young world following in the same path which those now old have followed, and so reaching the same condition. If the moon is really a much older world than the earth—and we have seen that in all probability she is—then she presents to us a picture of the condition which our earth will hereafter attain.

It may be well to mention here, but to mention only, the facts which are held by astronomers to show that the moon has a very rare atmosphere and no water-surface. In the first place, then, we know that, if the moon's atmosphere were not exceedingly rare, we should never see the sun totally eclipsed, some portion of the sun's light being brought into view round the moon's edge, even at the time of central eclipse, by the refracting power of the atmosphere. Nothing of this kind happens, however. It might once have been thought that the ruddy light of the prominences was the refracted sunlight, but the spectroscope has shown beyond all possibility of question that this is not the case. Secondly, the moon, as she moves over the heavens, blots out stars from view, and, advancing, brings them again into view, quite sharply and suddenly. If she had an atmosphere which was not extremely rare, they would be gradually obliterated, and as gradually reappear. Next, the shadows of the lunar mountains are appreciably black, whereas we know that an atmosphere (unless exceedingly rare) would bend and reflect the solar rays in such sort as to take off the blackness of the shadows. There is, fourthly, no appreciable twilight in the moon, the boundary line between light and darkness being always sharply defined: now we receive light after sunset from portions of our atmosphere fully forty-five miles above the sea-level, where the atmospheric pressure is probably scarcely the eight-thousandth part of that at the sea-level. And though it would not be safe to infer that the lunar air is so rare as that, seeing that one or two observers have suspected the existence of a twilight circle on the moon, yet it may quite safely be concluded, from this and the other considerations named above, that the lunar atmosphere must have a very small density Probably the density may be about one five-hundredth part of the density of the air we breathe at the sea-level; but at the highest possible estimate the density of the lunar atmosphere cannot exceed the two-hundredth part of the density of the air we breathe. With an atmosphere such as this, the boiling point would be very close to freezing point, and the gentlest warmth from the rays of the rising sun would suffice to boil water away—that is, to turn it entirely into vapour. It will be understood, then, how utterly impossible it is that even the smallest water-surface should continue under the rays of the lunar mid-day sun—day lasting on the moon about a fortnight of our time.[1]

[1] Mr. Boyle, a telescopist of New York, announced some few years since that he had detected pools of water upon the moon, having doubtless been deceived by some of those bright spots

The theory advanced by Frankland in this country respecting the way in which the lunar air and seas have been caused to disappear must now be considered. In passing, I may remark that the originator of the theory was Seeman, the German geologist; but it was independently advanced by Frankland in England, Stanislas Meunier in France, and Sterry Hunt in America.

In the first place, it is to be noted that no other theory seems available. Of three others which have been advanced, only one, Hansen's, according to which the seas and atmosphere of the moon have been drawn by lunar gravity to the farther or unseen hemisphere of the moon, needs serious refutation. (The other two are Whiston's theory, that a comet carried off the lunar seas and air; and the theory—whose author is unknown to us—that the lunar seas, and later the lunar atmosphere, have been frozen through the intensity of cold to which, in the long lunar nights, the moon is exposed.) But this theory is no longer entertained by

which seem to possess a mirror surface. It will be seen from what is stated above that no such pools could possibly remain as such under the sun's rays, even assuming that such pools might exist during the lunar night, when, however, the temperature would necessarily be far below the freezing point. Some, by the way, suppose that the question whether any processes resembling vegetation can take place on the moon is on the same footing as the question whether small portions of the moon's surface can be covered with water. But this is a mistake. It is altogether improbable, no doubt, that there is any vegetation on the moon, but it is not absolutely impossible, whereas it is utterly impossible that any water-surface could be seen there. Whatever water may have been imagined by telescopists during their surveys of lunar scenery has been as unreal as the water sometimes seen by travellers on African deserts.

astronomers, simply because it has been shown that the peculiarity of the moon's shape which had suggested the theory has been found, first, to have no real existence; and, secondly, to be incapable, if it existed, of exercising the supposed effect.[1]

The theory independently advanced by the four students of science named above is simply this, that seas formerly existing on the surface of the moon have

[1] The idea was that the moon, though nearly spherical, is somewhat egg-shaped, the smaller end of the egg-shaped figure being directed towards our earth. Now, while it is perfectly clear that on this supposition the greater part of the moon's visible half would be of the nature of a gigantic elevation above the mean level, and would therefore be denuded (or might be denuded) of its seas and the denser parts of the air formerly covering it, yet it is equally clear that all round the base of this monstrous lunar elevation the seas would be gathered together, and the air would be at its densest. But it is precisely round the base of this part of the moon, or, in other words, round the border of the visible lunar hemisphere, that we should have the best chance of perceiving the effects of air and seas, if any really existed; and it is because of the absolute absence of all evidence of the kind that astronomers regard the moon as having no seas and very little air. It is worthy of notice that Hansen's theory was anticipated by the author of that clever little pamphlet called *The Lunar Hoax*, who places the human inhabitants (the Bat-men) in the regions near the edge of the lunar disc, on the strength of some such views as Hansen advanced a quarter of a century later. Recently the *Chicago Times* published several columns of lunar-hoax matter, purporting to be an account of observations made in France with a new and exceedingly powerful reflecting telescope. The observations made with this instrument showed a number of lunar folks, whose movements rendered it manifest that they were prisoners undergoing some kind of penal servitude, the visible lunar hemisphere being a sort of Botany Bay or Cayenne for lunar offenders, while the other hemisphere is a comfortable place of abode for good moon people. But what an unhappy state of things is here suggested! Conceive a world one-half of whose surface is required as an abode for its malefactors!

been gradually withdrawn into the moon's interior, and that a similar process, but chemical rather than mechanical, has led to the withdrawal of the greater portion of the air which formerly enveloped the moon's frame.

It may be well, first, to inquire whether the moon is likely to have had originally an atmosphere of considerable density and oceans of considerable extent. Supposing, for the sake of argument, that the materials of the moon's mass (including air and water) were originally proportioned as to quantity very much like those of our earth's mass, it is easily seen that the quantity of air above each square mile of the moon's surface, at the time when the moon had reached the stage of planetary development through which our earth is now passing, must have been very much less than the quantity of air now existing above each square mile of the earth's surface. For, the moon's mass being about an eighty-first part of the earth's, the mass of the lunar air must have been about an eighty-first part of the mass of our present atmosphere. But the moon's surface bears a much greater proportion to the earth's, being about a thirteenth. Whence it follows that, on the assumptions we have made, the quantity of air above each square mile of the moon's surface would be only about one sixth part of the quantity above each square mile of the earth's surface. And this air being drawn downwards only by lunar gravity, which has but about a sixth part of the energy of our terrestrial gravity, would be less compressed in the same degree on this account. One-sixth of the quantity of air being thus compressed with one-sixth the amount of force, it is clear that the density of the lunar air in that stage of the moon's existence would only be about one thirty-sixth of the density of our air. Similar reasoning applies to the water, except as to the compression under lunar gravity. The average quantity of water to each square mile of the moon's surface would be but about one sixth part of the quantity there is for each square mile of the earth's surface. The relative extent of the lunar oceans would not be less in precisely the same degree, however. For, speaking generally, the bed of the ocean slopes downwards from the shore-line in such a way that more than half, or a third, or a fourth, or so on, would have to be removed to diminish the surface by a half, a third, or a fourth, or so on, respectively. We may illustrate our meaning here by considering the relation between the quantity of water in a wine-glass (supposed to be cone-shaped) and the surface of the water. Suppose the wine-glass full at first, and the circular surface of the water to be three square inches, then if five-sixths of the water are thrown out, so that only one-sixth remains, the surface will not be reduced to one-sixth its former extent—that is, to one-half of a square inch—but will be about nine-tenths of a square inch. It is clear that in the case of an ocean having a bottom very steeply sloping near the shore-line, and nearly level elsewhere, a large proportion of the water might be drawn off, and the ocean-surface still remain almost as great as before. We may assume as a mean and sufficiently probable hypothesis that the lunar oceans had a relative surface equal to between one-half and one-third of the present relative surface of the terrestrial oceans. That is to say, our oceans covering about 72 hundredths of the entire surface of the earth, we may assume that the lunar oceans covered between 36 and 24 hundredths of the entire surface of the moon. It will be seen presently that some importance attaches to this question of the probable surface of the seas on the moon, a portion of the evidence for the theory we are examining depending on this relation.

Let us next consider in what way the withdrawal of the lunar oceans into the moon's interior probably took place. On this point, Frankland's presentation of the theory is undoubtedly defective. In fact, it has been the weakness of the theory in this respect, as presented in England, which has in all probability prevented it from receiving the attention here which it fairly deserves. 'The cooling of the moon's mass must,' said Frankland, 'in accordance with all analogy, have been attended with contraction, which can scarcely be conceived as occurring without the development of a cavernous structure in the interior. Much of the cavernous structure would doubtless communicate, by means of fissures, with the surface, and thus there would be provided an internal receptacle for the ocean, from the depths of which even the burning sun of the long lunar day would be totally unable to dislodge more than traces of its vapour.' And he proceeds thus to analyse the amount of space which would be rendered available for the retreat of the lunar oceans. 'Assuming the solid mass of the moon to contract on cooling at the same rate as granite, its refrigeration through only 180° of the Fahrenheit thermometer (the difference between the boiling and the freezing points) would create cellular space equal to nearly $14\frac{1}{2}$ millions of cubic miles, which would be more than sufficient to engulf the whole of the lunar oceans, supposing them to bear the same proportion to the mass of the moon as our own oceans bear to that of the earth.'

But in reality no such cavernous structure could possibly be developed in the interior of a planet like the moon. Frankland's mistake, here, is similar to that made by Brewster and others, who have suggested that possibly the small mean density of the outer planets might be due to the existence of great void spaces in the interior of those bodies. So soon, however, as we make the roughest calculation of the pressures existing in the interior of even a small planet like the moon, we perceive that there could be no cavities. The most solid materials—steel, adamant, platinum—become plastic under pressures far less than those brought into action by the attractive energy of a planet's mass upon all parts of its interior, except those not far from the surface. Be it noticed that it is not, as some seem to suppose who have written on this subject, the force of gravity at different depths which has to be considered. *That* diminishes as the centre of the planet is approached. What we have really to consider is the pressure produced by the weight of the superincumbent mass above any given level, and this of course becomes greater and greater as the depth below the surface increases. If the rigidity of the solid substances forming the solid crust of a planet were such that any amount of pressure could be borne without impairing it, then of course the various layers of the crust would form a series of arches, stronger and stronger with approach to the centre, because of the increased compression, and therefore the increased density of their substance. There is no *à priori* reason, perhaps, why this should not be so. Compression, for example, *might* increase the rigidity or force-resisting power of the materials of the earth's substance in such sort that mines might be dug to any depth, and horizontal tunnelling carried out from the lowest parts of any mine. But experiment shows that the fact is otherwise. Under great pressures the most solid substances become plastic. Steel behaved like a liquid in Tresca's experiments, affording the most conclusive evidence that at a depth of ten or twelve miles no steel walls, however massive, could defend a cavernous space from the surrounding pressures, which would simply crush in the steel until it formed one solid mass without interstices—at least with no interstices which could be seen if the steel were afterwards brought up from that depth to be cut open and examined. It will be readily understood that at the depth of ten or twelve miles there can be no caverns into which the water of the oceans could be bodily withdrawn. Extending similar considerations to the moon, we perceive that there can be no caverns in the moon's interior at a greater depth than sixty or seventy, or at the utmost 100 miles. Now 100 miles is less than the twentieth part of the moon's diameter, and the entire mass of the moon exceeds the mass of the outermost layer (to a depth of 100 miles) in about the proportion of four to one. So that even on the assumption that all the external parts of the moon, to the depth of 100 miles, contracted in such a way as to leave cavernous spaces in the manner conceived by Frankland, there would not be nearly enough space for the lunar oceans, supposing them to bear the same proportion to the moon's mass which our ocean bears to the mass of the earth.

But, though cavernous spaces would not form throughout the interior of a planet, room would yet be found, even to the degree conceived by Frankland, for the waters of the planet. The greatest possible pressure to which the most solid rock can be exposed would not fill the capillary spaces which exist throughout the material of the rock, while the pressure on the water at great depths would force it into even minuter than capillary spaces. This has been conclusively shown during experiments entered upon for another purpose—viz. to determine the compressibility of water. For when in 1661 Florentine academicians tried to compress water which had been enclosed within a globular shell of gold, they found that the water under great pressure forced its way through the pores of the gold, and stood on the outside of the globe like dew; and since that time the experiment has been repeated with globes of other metals, a similar result being obtained.

It follows from these considerations that, as a planet cools, more and more space is formed for the retreat of the planet's seas; and that in all probability in the extreme old age of a planet, when its whole frame to the very centre has been sufficiently cooled, space enough is thus formed to hold all the water which had once adorned the planet's surface.

If we consider the whole history of the moon's cooling, partly as indicated by her actual aspect, partly by the evidence given by the aspect of other planets, and partly as justly inferrible from the laws of physics, we shall find abundant reason for believing that *her* seas at any rate might thus have been withdrawn. During the earlier stages of a planet's history, considered in the essay 'When the Sea was Young,'[1] the seas are floating in the form of cloud and vapour above the planet's surface. In the next stage, when the crust is still hot, but not too hot for the waters to rest upon it, the process of cooling must take place more rapidly in the crust of the planet than in the planet's interior. All this time, then, the crust would be contracting upon the nucleus—a process which would leave no cavernous spaces between the crust and the nucleus for the waters to retreat to. From time to time the contracting crust would give way, exactly as a non-contracting crust would give way under the pressure of an expanding nucleus. The scene of such a catastrophe

[1] See p. 77.

would be marked thereafter by a great crater at the place where the crust first gave way, and a series of radiating streaks marking the places where the crust was split open all around that spot. The signs of events such as these in the moon's earlier history are very manifest. There is the great lunar crater Tycho, which is clearly visible to the naked eye, near the lower part of the disc of the moon; and from this as a centre radiations extend in all directions, some of which run right across the visible lunar hemisphere, and probably extend right round the moon. These also can be seen with the naked eye; and they are so well marked in photographs of the moon that some supposed the earlier photographs by Draper and Rutherfurd in America, and by De la Rue in this country, were in reality only photographs of a peeled orange, the crater Tycho representing one end of the core, and the radiations corresponding to divisions between the sections of the orange. Besides this most remarkable case, there are six others, centres of radiating streaks on the moon's visible hemisphere, and doubtless others upon the unseen hemisphere. We have here clear evidence of the tremendous nature of the forces which were at work throughout the moon's frame in the earlier stages of her history, the disturbance in particular by which the radiations from Tycho were made having apparently wracked the whole frame of the moon. Directly, indeed, these considerations do not affect the theory we are considering, because no large portion of the lunar seas can by any possibility have retreated beneath the surface during this stage of her existence. But as showing the enormous store of heat which existed at that time (by far the larger part of which must have remained unexhausted when the next stage began), the consideration of these amazing evidences of disturbance has an important though indirect bearing on our subject.

After the crust had parted with the greater portion of the heat which it had possessed when first formed, it would cool, and therefore would contract but slowly. The nucleus, on the other hand, which had before contracted more slowly than the crust, would now contract more rapidly, leaving spaces between itself and the crust. And then two things would happen. One would be the manifestation of vulcanian energy in consequence of the heat generated by the crust as it crushed its way downwards upon the retreating nucleus. The other would be the influx of water wherever it found access to the cavernous spaces between the crust and the nucleus. It is probable that before this vulcanian era of the moon's history was completed a considerable portion of the lunar waters had taken its place permanently beneath the crust. It should be noticed that this era corresponds with a part of the earth's existence which is as yet far from being completed, even if it can be regarded as much more than begun. It is far from unlikely that the era during which a planet's crust is thus kept in constant activity by the retreating motion of the nucleus synchronises with the period during which life exists on the planet's surface. During all this period, which may have lasted tens of millions of years, not only were portions of the waters of the moon gradually taking up their place in cavernous spaces between the crust and the retreating nucleus, but another process must have been at work to exhaust the lunar seas. When water falls upon a large land-surface in the form of rain, so that the surface is thoroughly drenched, a portion probably disappears permanently from the water-circulation of the globe. Of course, the greater portion is conveyed into the sea in the form of running water. Then, again, the drying of the surface means that the water which had moistened it is taken into the air again in the form of aqueous vapour. And this eventually assumes the form of visible cloud, and after sundry changes (during which it may many times in turn appear as cloud or disappear as vapour) it falls again in rain, and *may* be either restored in this way directly to the sea from which it came, or so fall on land-surface as to run into some stream communicating by brook, rivulet, river, and estuary with the ocean. And some portion of the water which falls on land-surfaces, passing below the surface, feeds internal streams, and eventually appears again in the form of spring-water. But it cannot be doubted that a portion of the water which falls on dry land soaks its way downwards, very slowly, perhaps, but steadily and continuously, thus removing itself from sight, and *pro tanto* diminishing the planet's surface-waters.

How much of the water would be removed by these causes, before the last stage of all began (at least the last change of a planet's existence as a body undergoing change) is not easily determined. Probably a quarter or a third of the water forming the original oceans of a planet might be withdrawn in one or other of these ways, leaving the rest to be removed during the refrigeration of the nucleus itself—a process requiring many millions, possibly hundreds of millions, of years for its completion.

In whatever way the withdrawal of the lunar seas was accomplished, it is certain that every particle of water has disappeared from the surface of the moon; and as there are clear signs of the former existence of extensive lunar seas, apart from the strong *à priori* considerations showing that the moon must once have had water on her surface, we have little choice but to admit that the waters of the moon have been withdrawn by such gradual processes as have been described above, and consequently that the era of the moon's existence as a habitable world is really removed from the present epoch by the enormous time-intervals required for the completion of those processes. In fact, we can see clearly pictured on the moon's face the evidence which shows that she has passed through all the stages of planetary life, from the time when her whole frame was glowing with intensity of heat, down to the period when she had reached the condition which our earth in the remote future must attain—that of a cold dead orb, neither living itself (regarding physical changes as corresponding with vitality) nor capable of being the abode of living creatures. Extending the range of our survey, we find in the giant planets, Jupiter and Saturn, the evidence of an earlier stage than any of which the moon's present aspect affords direct evidence. The sun presents a yet earlier stage, while the gaseous nebulæ or masses of luminous star-vapour scattered through the immensity of space illustrate the earliest of all stages of cosmical existence of which we have any direct evidence. On the other hand, we see in Mars, with his small ocean-surface and rare atmosphere, the picture of a stage intermediate between that through which the earth is now passing, and the decrepit or death-like condition of the moon. Mercury, if we could examine his condition more satisfactorily than is the case, would probably illustrate a stage somewhat nearer to the moon's present condition. Venus, on the other hand, so far as can be judged, though a somewhat smaller planet than the earth, is in a somewhat earlier stage of planetary existence.

Although the moon may be regarded as to all intents and purposes dead, it must not be supposed that no changes whatever take place upon her surface. On the contrary, some of the peculiarities of the moon's condition must tend to cause even more rapid changes of certain orders than take place in the case of our own earth. Thus the great length of the lunar day, and the moon's waterless condition and rare atmosphere, must help to cause a comparatively rapid crumbling of the moon's surface. During the long and intensely hot lunar day the rock substance of the moon's surface must expand considerably, for it is raised to a degree of heat exceeding that of boiling water. During the long lunar night the surface is exposed to a degree of refrigeration far exceeding that of the bitterest winter in the Arctic regions, and must contract correspondingly. This alternate expansion and contraction must gradually crumble away all the loftiest and steepest portions of the moon's surface, and will doubtless, in the long run —that is, some few hundreds of millions of years hence —destroy all the most marked irregularities of the moon's surface.

The cases of change which have been recognised by telescopists who have carefully studied the moon's surface may all, without exception, be referred to this process of gradual but steady disintegration. The most remarkable case hitherto known, for example, the disappearance of the lunar crater Linné, is far better explained in this way than as the result of volcanic outburst. This case has recently been described as follows, by the present writer:—In the lunar Sea of Serenity there was once a deep crater, nearly seven miles across, a very distinct and obvious feature, even with the small telescope (less than four inches in aperture) used by Beer and Mädler in forming their celebrated chart. But, ten years ago, the astronomer Schmidt, a selenographer of selenographers (who has in fact given the best energies of his life to moon-gazing), found this crater missing. When he announced the fact to the scientific world, other astronomers, armed with very powerful instruments, looked for the crater which had been so clearly seen with Mädler's small telescope; but though they found a crater, it was nothing like the crater described by Mädler. The present crater is scarcely two miles in diameter, and only just visible with powerful telescopes; all around it there is a shallow depression, occupying a region about as large as the whole crater had been before. It seems impossible to doubt that a great change has taken place here, and the question arises whether the change has been produced by volcanic activity or otherwise. Sir John Herschel pronounced somewhat confidently in favour of the former hypothesis. 'The most plausible conjecture,' said he, 'as to the cause of this disappearance, seems to be the filling up of the crater from beneath, by an effusion of viscous lava, which, overflowing the rim on all sides, may have so flowed down the outer slope as to efface its ruggedness, and convert it into a gradual declivity casting no stray shadows.' 'But how tremendous the volcanic energy,' we note in the passage referred to, 'required to fill with lava a crater nearly seven miles in diameter, and more than half a mile deep! The volcanic hypothesis seems on this account utterly incredible, for if such energy resided in the moon's interior we should find her whole surface continually changing. Far more probable seems the idea that the wall of this crater has simply fallen in, scattering its fragments over what had once been the floor of the crater. The forces at work on the moon are quite competent to throw down steep crater-walls like those which seem formerly to have girt about this deep cavity.' [1]

That the kind of vitality evidenced by such changes as these still exists in the moon's frame, is not merely probable but certain. Other changes, however, which were once supposed to have been observed, must be dismissed as having had no real existence. The effects of various kinds of illusion have to be taken into account in considering such phenomena. Thus the theory that a process of monthly change, due perhaps to vegetation, affects the floor of the large lunar crater Plato (called by Hevelius the greater Black Lake), is now rejected, because the supposed change has been shown to be a mere effect of contrast. The apparent change is of this nature:—As the sun first begins to rise above the floor of the crater—or, in other words, as the light of the filling moon gradually flows over the crater—the floor appears bright, getting brighter and brighter as the sun rises higher and higher, up to a certain point. But afterwards the floor darkens, becoming darkest towards lunar midday. Lastly, as the lunar afternoon progresses, the floor of Plato gets gradually lighter again. The mid-day darkening was attributed to some process of vegetation or else to chemical changes. It has no real existence, however, but is due simply to the effect of contrast with the great brightness of the crater-wall all around, which is formed of some very white substance, and looks peculiarly bright and lustrous at the time of lunar mid-day,

[1] The present writer, in the *Spectator* for June 24, 1876.

so that contrasted with it the floor looks peculiarly dark. On the other hand, during the morning and evening hours, the black shadow of the crater-wall is thrown across the floor, which by contrast looks brighter than it really is. This explanation has indeed been denied very confidently by some who formerly advocated the theory that lunar vegetation causes the darkening of the floor; but there can be no doubt of its justice, for no one (not prejudiced in favour of a theory) who has tested the matter experimentally, eliminating the effects of contrast, has failed to find that there is no real darkening of the floor of Plato.

It seems as certain as any matter not admitting of actual demonstration can be that the moon is, to all intents and purposes, dead. Her frame is indeed still undergoing processes of material change, but these afford no more evidence of real planetary life than the changes affecting a dead body are signs of still lingering vitality. Again, it seems certain that the processes through which the moon has passed in her progress towards planetary death, must be passed through in turn by all the members of the solar system, and finally by the sun himself. Every one of these orbs is constantly radiating its heat into space, not indeed to be actually lost, but still in such sort as to reduce all to the same dead level of temperature, whereas vitality depends on differences of temperature. Every orb in space, then, is tending steadily onwards towards cosmical death. And, so far as our power of understanding or even of conceiving the universe is concerned, it seems as though this tendency of every individual body in the universe towards death involved the tendency towards death of the universe itself. It may indeed be said that since the universe is of necessity infinite, whereas we are finite, we cannot reason in this way from what we can understand, or conceive, to conclusions respecting the universe, which we cannot even conceive, far less understand. Still it must be admitted that, so far as our reasoning powers can be relied upon at all, the inference, from what we know, appears a just one, that the life of the universe will have practically departed when the largest and therefore longest-lived of all the orbs peopling space has passed on to the stage of cosmical death. So far as we know, there is but one way of escape from this seemingly demonstrated, but in reality incredible, conclusion. May it not be that as men have erred in former times in regarding the earth as the centre of the universe, as they have erred in regarding this period of time through which the earth is now passing as though it were central in all time, so possibly they may have erred in regarding the universe we live in, and can alone comprehend, as though it were the only universe? May there not be a higher order of universe than ours, to which ours bears some such relation as the ether of space bears to the matter of our universe? and may there not, above that higher order, be higher and higher orders of universe, absolutely without limit? And, in like manner, may not the ether of space, of which we know only indirectly though very certainly, be the material substance of a universe next below ours,[1] while below that are lower and lower orders of universe absolutely without limit? And, as the seemingly wasted energies of our universe are poured into the universe next below ours, may it not well be that our universe receives the supplies of energy wasted (in seeming) from the universe next in order above it? So that, instead of the absolute beginning and the absolute end which we had seemed to recognise, there may be in reality but a continual interchange between the various orders of universe constituting the true universe, these orders being infinite in number even as each one of them is infinite in extent. We find ourselves lost, no doubt, in the contemplation of these multiplied infinities; but we are equally lost in the contemplation of the unquestioned infinities of space and time amidst which our little lives are cast, while the mystery of infinite waste, which seems so inscrutable when we consider the universe as we know it, finds a possible interpretation when we admit the existence of other orders of universe than the order to which our lives belong. Thus should we find a new argument for the teaching of the poet who has said—

> Let knowledge grow from more to more,
>   But more of reverence in us dwell,
>   That, mind and soul according well,
> May make our music as before,
> But vaster;

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[1] The work called the *Unseen Universe* presents a portion of the evidence to this effect, but unfortunately the style of that work is not sufficiently lucid to bring its reasoning within the range of the general non-scientific reader.

a new significance in the vision of him who said—

> See all things with each other blending,
> Each to all its being lending,
> All on each in turn depending;
> Heavenly ministers descending,
> And again to heaven uptending,
> Floating, mingling, interweaving,
> Rising, sinking, and receiving—
> Each from each, while each is giving
> On to each, and each relieving
> Each—the pails of gold; the living
> Current through the air is heaving;
> Breathing blessings see them bending,
> Balanced worlds from change defending,
> While everywhere diffus'd is harmony unending.

THE MOON'S MYRIAD SMALL CRATERS.

Since Galileo first turned a telescope upon the moon, the lunar craters have been among the wonders and mysteries of astronomy. It is not merely or even chiefly the vast size of some of these objects which excites astonishment. Indeed, it might almost be inferred from what we know of the moon's size and general structure, that her volcanic energies would be more effective, though not greater, than those of our own earth. The really surprising characteristic of the lunar surface is the amazing number of the lunar craters. Even Galileo, though with his weak telescope he could see but a few of the craters which really exist in the moon, compared those in the south-western part of the moon's disc to the eyes in a peacock's tail. With each increase of telescopic power, more and more craters have been seen. Regions supposed to be comparatively smooth have been found, on closer scrutiny with higher powers or under more favourable conditions, to be covered with minute craters. The slopes of the larger craters, even in some cases their floors, have been found to be strewn with small crater-shaped depressions. In fine, almost the whole surface of the moon may be said to be pitted with depressions of all sizes, from mighty gulfs three or four hundred miles across, down to minute saucer-shaped shallows, such as only the most powerful telescopes will reveal.

I propose to enter here into a brief consideration of the probable cause of the smaller lunar craters. Unquestionably the feature may be regarded as marking a characteristic distinction between the moon and our own earth. It may well be that the moon is an old world, while our earth is comparatively young; but, for my own part, I cannot consider that the earth can come during the progress even of millions of years to resemble the moon in details, however closely she may hereafter resemble the moon in general respects—in the absence of water, for instance, in the tenuity of her atmosphere, and so forth.

The course I propose to follow is one which, I think, may with advantage be pursued in a great number of cases in which as yet it has been little followed. Starting with the views now generally entertained respecting the origin and structure of the solar system, I propose to inquire what might in all probability be expected to happen in the special case of our own moon; comparing the results to which we seem led, in this way of viewing the matter, with the results of actual observation. In other words, I am going to follow an *à priori* method of reasoning, testing the conclusions to which it may lead by *à posteriori* considerations.

It is now generally admitted that the various members of the solar system reached their present condition by processes of development. Few, however, among those who have studied the theory of cosmical evolutions for themselves, are disposed to accept unquestioningly Laplace's idea that the whole solar system was once a great mass of gaseous matter. It is only, indeed, by carefully closing the mental eye to the results of modern physical researches, that a theory of the kind can for a moment be entertained. I will not here consider the multitudinous objections against the so-called nebular hypothesis, regarded as the sole hypothesis of the origin of the solar system. Nor, on the other hand, will I consider here in detail the arguments in favour of the theory that the various members of the solar system acquired no small portion of their present bulk by a process of aggregation. Let it suffice to mention that the theory of planetary and solar growth, by the gathering in, during past ages, of immense quantities of meteoric and cometic matter, is one which has this immense advantage over the nebular theory, that it assumes the former action of a process which is going on at this present time; while also, as regards the materials forming the masses of the sun and planets, this theory leads to inferences according well with known facts.

I must, however, premise that neither the aggregation theory alone nor the condensation theory alone can fully explain the observed present condition of the solar system. We must admit on the one hand that the several members of this system, including the sun, gathered in their substance in large amount from without. But we must also admit the former vaporous condition of the sun and planets, not indeed exactly in the way indicated by Laplace, for these bodies never could have had the enormous extension his theory required and yet have retained coherence; but that they were formerly far more expanded than at present, and were thus of very small density, may be regarded as to all intents and purposes certain. Indeed, the aggregation theory would be insufficient to account for the formation of even a small portion of each planet's mass, unless we remembered that in the earlier stages of their existence the several planets were vaporous, and therefore much larger than in their later solid condition. For it would only be when thus expanded that they would gather, in their orbital motion around the sun, a sufficient quantity of meteoric or cometic matter. At present, for instance, our own earth, though she gathers in some 400 millions of meteors in the course of each year, yet gathers a quantity of matter so small compared with her own substance that in the course of 400 millions of years the earth's diameter would be increased only by a single inch. When the earth had a much smaller mass than she has now, however, but that mass vaporous and of small density, she would gather in many thousand times as much matter in each circuit round the sun, apart always from the fact that in those remote times the quantity of meteoric matter as yet not gathered in was many thousand times greater than it is at present.

Now, we have in considerations such as these the means of explaining in some degree the peculiarities of the moon's state.

In the first place, we must set the period during which the moon's globe was being fashioned by cosmic forces in a far more remote antiquity even than the corresponding period of the earth's history. How far back the last-named period should be set is not very easily guessed even in the roughest manner. According to geologists, the interval during which the earth's crust has in general respects been in the same state as at present, must lie between 400 million years and 20 million years. The preceding period, during which the crust was cooling from the heat it possessed when first formed to a temperature such that living creatures could exist upon the crust, must have lasted at least 300 millions of years. The period preceding that again, when the earth had no crust, but was almost entirely vaporous, lasted probably many hundred millions of years. It must have been during this remotest of all the periods of the earth's own history that the moon was formed. But she must have been detached from the earth's mass, or rather left behind by the retreating vaporous mass of the earth, very early in this first stage of the earth's existence.

Whether at this time the moon (which in any case contained far less matter then than she does now) existed as a single mass or as a number of small masses scattered round a ring-shaped region, is a point on which different views may be entertained. For my own part, though I cannot doubt that the substance of the moon once formed a ring around the earth, I think there is good reason for believing that when the earth's vaporous mass, receding, left the moon's mass behind, this mass must already have been gathered up into a single vaporous globe. My chief reason for thinking thus is, that I cannot on any other supposition find a sufficient explanation of one of the most singular characteristics of our satellite—her rotation on her axis in the same mean time, exactly, as she circuits around the earth.

This peculiarity in the moon's rotation is generally treated as though it were a natural and, so to speak, an antecedently likely arrangement, instead of being one of a very remarkable and unlikely nature. It is stated, very justly, that if the moon's original rate of turning had nearly coincided with her rate of travelling round the earth, in such sort that she would very nearly keep one side directed towards the earth during a single revolution, the earth's attraction on the elongated body of the moon would so operate as to compel the moon always to keep that side earthwards. The longer axis of the moon would sway backwards and forwards on either side of a line directed towards the earth, but would not be carried altogether round so as to bring the farther side of the moon eventually into full view. And as we know that such swayings, if they really take place, are very slight (for what is called the moon's libration or balancing has nothing to do with the swaying I refer to), it follows that originally the moon's rotation must have agreed very closely indeed with her rotation. All this is correct enough; but what is commonly left unnoticed is the exceedingly improbable nature of the imagined coincidence, if the moon's rate of rotation and her rate of revolution had been independently communicated to her.

Professor Grant, in his fine work the 'History of Physical Astronomy,' speaks of this coincidence as a relation which, though difficult to explain by the doctrine of chances, becomes very interesting and suggestive when it is considered as the result of Supreme Intelligence. But that method of dealing with the difficulty is not likely to be acceptable in these times, when men regard all the facts ascertained by observation as belonging to the domain of science. There is not a single department of scientific research in which men might not be checked at the outset by an explanation of that sort. Newton asked, Why does the moon travel round the earth and the earth round the sun? and he proceeded in the scientific manner to find out. If he had been contented to answer, It pleased the Supreme Intelligence that these bodies should move precisely as they do, he would have manifested the fullness of his faith, but he would have lost the opportunity of effecting a very noble discovery, one too which affords grander conceptions of the mechanism of the universe than the mere motions which it explains. So here, in the case of the moon's rotation, it sounds well, perhaps, to say that we accept the observed fact as evidence of the wisdom of the Supreme Intelligence, and do not seek to know how it was brought about; but this submission of the intellect to faith implies not only a certain intellectual languor, but also a doubtful, hesitating faith, I confess that for my own part I prefer the honest bluntness with which my valued friend Professor Newcomb presents this matter. 'That the adjustment,' he says, 'should be a mere matter of chance, without any physical cause to produce it, is almost infinitely improbable, while to suppose it to result from the mere arbitrary will of the Creator, is contrary to all scientific philosophy.'

Now, there is a circumstance in the condition and movements of our own earth indicating a way by which the moon might have attained that peculiar rate of rotation. The tidal wave, which, roughly speaking, may be said to sweep twice a day round the earth in a direction contrary to her rotation, exerts a certain exceedingly small effect in slowing her rotation-rate, and thus in lengthening her day. This effect is so small that many millions of years must elapse before the day would be doubled in length, and many millions of millions of years before the earth would turn at such a rate as to present always the same face towards the moon, even if the present lengthening of the day continued constantly, instead of gradually diminishing from its present exceedingly minute amount. Now, if we suppose the moon to have existed for millions of millions of years, and to have had during the greater part of that time a deep ocean in which tides would be raised by the earth's attraction, we can understand the possibility that an original rotation of the moon at something like the earth's present rate of turning might have been gradually reduced until at length the present slow rate of turning—once in $27\frac{1}{3}$ days—had been attained to. But we require most tremendous time-intervals on such a theory, and moreover we require that the moon's condition should at one time and for a long time have been exceedingly unlike her present condition. The former difficulty is more serious than the latter; for it is almost impossible to set back the formation of the moon farther than a few thousands, or at the most tens of thousands, of millions of years, whereas this theory would require that she should have been the scene of tidal disturbance during millions of millions of years.

If we suppose that her own mass was wholly or partially fluid for millions of years, we to some degree escape this difficulty, for the tides which would in that case have been raised by the earth would have been far larger than mere tides in the lunar seas. Formerly this was the explanation which seemed to me the most probable. I find that Professor Newcomb regards it with some degree of favour. 'If the moon were once,' he says, 'in a partially fluid state, and rotated on her axis in a period different from her present one, then the enormous tides produced by the attraction of the earth, combined with the centrifugal force, would be accompanied by a friction which would gradually retard the rate of rotation, until it was reduced to the point of exact coincidence with the rate of revolution round the earth as we now find it. We therefore see in the present state of things a certain amount of probable evidence that the moon was once in a state of partial fluidity.'

But while I still regard this theory as the true one, I recognise in a yet earlier stage of the moon's development the most effective part of the earth's action in modifying the rate of the moon's rotation. When the moon was in great part gaseous, at which time the earth was almost entirely gaseous, and probably extended beyond the mass whence one day the moon was to be formed, this mass would be compelled to rotate very nearly in the same time as it revolved around the earth's centre. It may be compared to a mass of matter carried round by a whirlpool. Such a mass might have a slow independent rotation in the fluid; but, speaking generally, we may describe its motion as corresponding to that which it would have if the fluid were so thick and viscid as only to allow the mass to move with it as it whirled round. If this were so in the moon's case, then when the contracting mass of the earth left the moon outside, the moon would have just such a rate of rotation as she has at present—that is, she would turn once on her axis as she circled once round the earth. And though, as the moon contracted, her rate of rotation would tend to alter, the action of the earth would be competent to overcome this tendency, compelling the moon to move always with the same face directed earthwards.

Though there are difficulties in the theory thus presented, and though indeed it is altogether unlikely that the exact correspondence described in the preceding paragraph ever really existed, I apprehend that there is no real objection to the theory that the observed peculiarity of the moon's rotation was chiefly brought about in this way—that is, while the moon's mass was in great part vaporous. In a later stage, when the moon's mass was chiefly fluid, another large share of the work would be done. Only a very small part would thus be left for the time when the moon's surface had become solid but was still swept by ocean tides. In this way we not only attain an explanation which accords with accepted views respecting the past condition of the moon, as one of the members of the solar system, but we escape the necessity of imagining periods of time so long that even the tremendous periods which science recognises as appertaining to the past of our solar system seem small by comparison. For it is certain that a globe like the moon, having oceans like those of our own earth, and rotating once in twenty-four hours, would not be compelled by the earth's attraction to rotate once a month in less than a trillion (a million million millions) of years.

It is well to notice, however, that no matter what physical interpretation of the observed peculiarity is accepted, we find in every case enormous time-intervals, during which the moon must have existed and have been subject to the earth's attraction. We are compelled to reject the idea that mere chance made the moon rotate as she does, keeping perfect time with her motion round the earth. We cannot accept the belief that, whereas the Supreme Intelligence allowed almost all the motions in the solar system to be completed in times no way related to each other, so that, for example, no exact number of days or months measure the year or any number of years, and that no exact number of hours or days measure the common lunar month or any other kind of month, or any number of any of these months, the Creator nevertheless saw fit in the Beginning to set the moon's turning motion in exact accordance with her motion round the earth—a relation not only utterly useless (at least, no one has ever yet been able to conceive any possible use it could have), but positively disadvantageous in more ways than one. It remains only that we should regard the relation as the result of physical processes: and so regarding it, we find that, in whatever way it was brought about, many millions or many hundreds of millions of years must have elapsed before the moon's movements received their final adjustment.

Now let us revert to the theory which I advanced originally in my book on the moon (p. 343, first edition), and which, as we have seen, Professor Newcomb considers the most probable—viz. that the moon's rotation-rate was determined when the greater part of her mass was fluid. Remembering the exceeding remoteness which must be assigned to that era of her career, let us consider the conditions under which she has existed since. It will be observed that I do not insist on her prior existence as a vaporous mass, at least as an essential point in my present reasoning. It is not that I entertain any doubt that she was for a long time a vaporous mass; but because it would be difficult to indicate any way by which any traces of what happened to her during that part of her existence could be detected. When she had become fluid, even, she would retain no trace of any of the accidents to which she would be exposed: luminous masses might fall upon her, but they would be absorbed into her fluid globe, leaving no sign of the encounter. It would not be till she began to lose her fluidity, as the fiery heat of her globe passed away, that any visible effects would result from the shocks and collisions to which she would be exposed. I pass on at once then to this era of the moon's existence.

It is certain, in the first place, that at that time millions of millions of tons of matter, now forming part of the masses of the various members of the solar system, were travelling about as meteors. It would be utterly unreasonable to imagine that the process of meteoric indraught at present taking place on the earth is not also taking place on every member of the solar system, or that this process of growth, which all the members of the solar system are undergoing now, has not taken place during past ages, and will not take place during ages yet to come. But this is far from being all. Since we know that every meteor that falls upon this earth, or on any other planet, or on the moon, is there and then brought to the end of its existence as an independent body, we perceive that the process of meteoric indraught is one of diminishing activity. The supply of meteors is becoming slowly but steadily exhausted. Doubtless plenty yet remain, and will remain for millions of years yet to come. They never can be all consumed, in fact, any more than the air in the receiver of an air-pump can ever be exhausted by the process of pumping. Each stroke of the pump removes a certain volume of the rarefied air left in the receiver; but as the air grows rarer and rarer the actual amount of air removed is diminished, and of course the air removed never can be the whole of the air left, since, by the very nature of the process of exhaustion, a small portion only of the contents of the receiver is removed at each stroke. So with the process of meteoric exhaustion. Every year the earth sweeps up or gathers in all the meteors encountered in its track, and each planet, in each of its circuits round the sun, does likewise; but as the meteors become rarer and rarer the number swept up in any given time becomes less and less. Nor can all ever be swept up, since each planet, in each of its circuits, clears of meteors only a very minute portion of the solar domain. The inference as to the past is obvious. Many millions of years ago the number of meteors gathered in by any planet or satellite must have been enormously greater than it is at present.

Now, the present rate of meteoric indraught is not altogether insignificant. It has been calculated that the earth gathers in, in the course of a year, as many as 400 million meteoric bodies, large and small, from the great masses which break their way through the air—our shield against the meteoric artillery—down to bodies so minute that a telescope would be required to make them visible in their rush through the air. This, be it remembered, is a result deduced from observation, and so deduced as certainly to fall short of the truth, not exceed it. In one sense the supply of meteoric matter seems enormous, while in another sense it is exceedingly small. If we assign to the meteors an average weight of only a single grain, we yet find that the earth grows a thousand tons in weight in three years, so that since the time of Abraham the earth's weight must have increased much more than a million tons. Probably one grain is too low an estimate of the average weight of these bodies. Professor Harkness, of Washington, has recently deduced from the known facts respecting meteors a result which accords closely with one which I myself enunciated in 1871 (as is natural, seeing that I used the same general evidence, and dealt with it in much the same manner). At the present rate of meteoric downfall, 400 million years or thereabouts would be required to increase the earth's diameter by a single inch.

It may seem at a first view as though this result were altogether inconsistent with the theory that any considerable portion of the earth's mass has been derived from meteoric aggregation. But in reality, when due account is taken, first of the former expansion of the earth's globe when it was in the vaporous state, secondly of the enormous length of time during which the process of indraught has probably taken place, and thirdly of the fact that the present density of meteoric distribution must be exceedingly small compared with that existing hundreds of millions of years ago, it appears that ninety-nine hundredths of the earth's whole mass might readily have been gathered in by meteoric aggregation. I do not here dwell upon the evidence showing this, because it does not belong to my subject; but it seemed necessary to mention that, so far is any difficulty from arising in the way suggested—that is, from the poverty of meteoric material—that in reality the real difficulty is to understand how the earth remained so small when we consider how enormous must have been the quantity of meteoric matter in remote eras to account for so many millions of millions of meteors remaining still uncaptured.

Now, the moon, travelling along with the earth in the remote ages to which our present inquiry relates, must have gathered in her own share of meteoric matter. At this present time, for instance, about thirty millions of meteorites, large and small, fall each year upon the moon. She passes through the same meteoric systems as the earth, and she can no more escape meteoric downfall as she thus rushes through these systems than the earth can. We may compare her companionship with the earth to that of a child with a grown person in a shower of rain. As many drops do not fall on the child as on the adult because the child is smaller; but the child gets as thoroughly drenched as his grown companion, assuming neither to be protected by an umbrella. So the moon receives as many meteors on each square mile of her surface (on the average of many millions of years) as the earth does. Since her surface is about one-thirteenth of the earth's (more exactly two-twenty-sevenths), she receives about one-thirteenth of the number of meteors which the earth encounters, or, taking the number above-mentioned for the earth, the moon's annual indraught of meteors is at present about 30 millions.

In passing, it is worthy of special notice that the downfall on each square mile of the moon is equal to the downfall on each square mile of the earth, on the average of long periods. It follows from this that the moon's present rate of growth from meteoric aggregation is equal to the earth's. Not that the moon grows equally either in volume or in mass, for her annual growth in both respects is but about one-thirteenth of the earth's annual growth; but as her surface is only a thirteenth of the earth's, a meteoric deposit of equal thickness is received each year by the moon and by the earth. And this has been true during millions of years past. Now if two bodies, unequal in size, were to grow equally in diameter year after year, they would become in the long run, to all intents and purposes, equal in size. Imagine a million miles added to the diameters of both the earth and moon; then the earth would have a diameter of 1,008,000 miles, and the moon a diameter of 1,002,200 miles, and these numbers are practically equal—the difference between them being very small compared with either. This is not a point of any importance as regards the future history of the earth and moon, for it is quite certain that neither will ever add half a mile to their present diameters, even though they should continue to travel as they now do for a million millions of years. But it is a point of extreme importance as respects the past of our earth and moon—a circumstance which, so far as I know, no one has hitherto noticed.

Suppose, for instance, we imagine the earth at some exceedingly remote epoch to have had only a thousandth of her present mass, so that at the same density her diameter would be only one-tenth that which she now has, and her surface one-hundredth of her present surface. Then if the moon existed at the same time, in the same state—vaporous, fluid, or solid—she would add as many miles to her diameter year by year from meteoric indraught as the earth would. And if this had continued to the present time, it would actually follow that the moon should have added to her diameter then (whatever it may have been) nine-tenths of the present diameter of the earth, or, roughly, about 7,000 miles. But the moon only has a diameter of about 2,160 miles altogether. It follows, therefore, that either the moon only had existence as a separate orb from the earth long after the earth had received the greater part of her present mass, or else the various stages of the moon's existence as a vaporous and as a fluid globe were very much shorter than the corresponding stages of the earth's existence. The latter is altogether the more probable explanation, and accords with what we should expect to happen during the cooling of the unequal masses of the earth and moon. But it is well to notice that our theoretical anticipations in this respect are thus confirmed by reasoning of another kind.

It has been calculated by Bischoff that the earth required 350 millions of years to cool from 2,000 degrees to 200 degrees centigrade, or in other words the earth must have existed as a ball of fused rocks for about that time. It may readily be shown that the moon would have remained fluid during only about a fourth of the time, say about 80 millions of years. Now, during the greatest part of this long period the surface of the moon would be viscid rather than fluid; and during the last ten or twelve millions of years of that period the moon's surface would be simply plastic. It would receive and retain any impressions which it might receive from without, much as the surface of a nearly dried pool of mud receives and retains the impressions of raindrops. Or rather, as such a surface, if stones be thrown upon it, allows the stones to pass through, and shows thereafter a shallow depression where the stone had fallen, so if any large mass fell upon the moon's surface while in the plastic state, the mass would pass below the surface, and a circular saucer-shaped depression only would show where the mass had fallen.

Let us suppose that the moon's surface was in this plastic state for only about three million years, remembering that, according to all that can be inferred from the experiments made by physicists and from the theoretical researches of mathematicians, this probably falls very far short of the truth.

And next let us suppose that at the remote era to which we must refer that special stage of the moon's development, the density of meteoric distribution in the solar domain was only ten times as great as it is at present, remembering that this also is probably very far short of the truth.

Now, among the meteors which fall each year upon the earth, few are large enough to break their way through the earth's atmospheric shield, without being either vaporised in their rush through it, or else caused to burst into a number of small fragments. Possibly over the whole earth some ten or twelve may thus fall in a year, one or two only being seen, because the chances are largely in favour of a meteorite escaping detection as it falls. If we suppose that at present only four such meteorites fall on the average each year upon the earth, and that therefore one only falls at present in the course of about three years upon the moon, we are certainly not taking an exaggerated estimate of the present rate of downfall of large meteoric masses upon our satellite. Of course a much larger number of meteoric bodies of all sizes reach the moon, for she travels on her course without the protection of an atmosphere, at least she has no atmosphere dense enough to ward off even the smallest meteors. So that, in reality, some 30 million bodies large and small must actually impinge on the moon's surface each year; and probably some ten or twenty thousand are of the kind we call fire-balls. It is, however, to be noted that almost every mass which thus strikes the moon must be vaporised by the intense heat excited as it impinges on the moon's surface; and even if this did not happen,[1] only one or two of the very largest which might so fall in the course of a century or so would be visible on the moon's surface observed under the most favourable conditions, with the largest telescopes made by man. Moreover, we may restrict our attention to the largest meteorites, in considering the moon's plastic era, for most probably at that time she had an atmosphere not far inferior to the earth's present atmosphere, as a shield against meteors.

Putting one very large meteorite in three years as the present rate of downfall on the moon, it would

[1] A certain proportion of meteoric masses reach the earth, and so, also, a certain proportion must reach the moon, with relatively small velocities. For instance, those which travel the same way, and either overtake or are overtaken with only the difference of their velocity and the velocity of the earth (or moon, as the case may be).

follow that, at the remote period to which our researches relate, ten such meteorites would fall in three years. Thus, in the three millions of years during which the era may be safely assumed to have lasted, ten million very large meteorites fell, according to the moderate assumptions we have made, upon the plastic surface of our satellite. These would not correspond to the very largest meteorites or aerolites known to men, either as having fallen on the earth or as seen and measured while moving athwart the sky. From time to time bodies are seen whose diameter is estimated at several hundred yards; and though no masses of this size have been known to reach the earth within the historic period, it must be remembered that the chances are usually in favour of the explosion of such meteorites into fragments as they pass through our air. I imagine, however, that the estimate of most of these bodies has been considerably exaggerated.[1]

The point to be noticed here, however, is this, that a mass far too small to be discernible at the moon's distance, would produce a discernible mark if it fell on the moon's surface in the plastic era. A circular depression far larger in diameter than the falling mass would be formed at the place where it had pierced the

[1] Though not quite to the extent imagined by Mr. Phipson in his treatise on Meteors, Aerolites, and Falling Stars. He has fallen into two mistakes, rather seriously affecting his conclusions: first, in taking the average height of great meteors above the earth as their average distance from the observer; and next, in supposing that a globe 206,000 times as far away as its diameter, subtends an angle of one minute, instead of an angle of one second only (a sixtieth part of a minute, that is).

viscous crust. So that we might fairly take into account the downfall of all the very large meteorites —that is, according to our estimate above, of some ten million masses—as competent to leave marks such as could be recognised with powerful telescopes from our earth, supposing nothing happened in later stages of the moon's history to obliterate such marks.

Among these ten million meteorites ten only in a thousand perhaps might be very large, so as to leave where they fell circular depressions from a quarter of a mile to a mile in diameter. For the diameter of the aerolites themselves, of course, would not be nearly so large as that of the circular depression left where they had fallen. In this case about a hundred million small shallow craters would be formed on the moon's surface during the plastic era.

But again, among these very large aerolites, probably some—it might be only one in a thousand—would be excessively large, from a quarter to half a mile perhaps in diameter. It is true, we know of no such mass having struck our earth within historic times, nor have any such masses been recognised in the earth's crust; but so many instances are on record of the passage of masses apparently as large as 100 yards in diameter through our air, which but for the air would certainly have fallen with their full mass on the earth's solid surface, that we cannot but believe in the existence even to this day of many enormous meteorites, and in the probability that at long intervals they fall upon our earth's atmospheric shield. Thus during these three million years some hundred very large masses would fall upon the moon's plastic surface, leaving where they had pierced the moon's crust vast circular depressions, each far exceeding in diameter the mass whose downfall had produced it.

Before proceeding to consider the result of such meteoric downfall on the moon's surface, I must remind the reader yet once more that, strange though these considerations which I am presenting to him may seem, they are based entirely upon known facts, and probably fall even far short of the truth. The nebular hypothesis, or some modification of that hypothesis, of the formation of the solar system is received by all astronomers of repute in the present day. The enormous duration of the various periods of planetary and lunar development has been demonstrated not only by experiments on the cooling of various substances, but by the study of our earth's crust. We know that meteors of all kinds still encounter the earth, and have no choice but to believe that, since so many now remain, the number existing millions of years ago must have been enormously greater. We know certainly that the moon in her journey round the sun must have encountered her share of these meteoric bodies. And we cannot possibly doubt that any considerable meteoric mass falling on the moon's surface at any time during the long period when that surface was wholly or partially plastic, would leave a larger circular depression where it has pierced the crust.

All these points may be regarded as certain; at least, any doubts respecting them must be doubts affecting the general theory of the evolution of the solar system, and such doubts need not here be combated.

But now the question arises whether the marks thus left upon the moon's surface would remain during the later stages of her existence down to the present time. It is certain that the surface of our own earth must once have been in a similar way pitted with the marks of meteoric downfalls, for she, like the moon, was in her growth

> Pelted with star-dust, stoned with meteor-balls,

and the era when her surface was plastic to receive and to retain the marks of the meteoric hail-storm (before

> Man and his works and all that stirred itself
> Of its own motion

could live upon it) lasted many millions of those cosmical instants which men call years. Yet we know that of those impressions which the earth then received no traces now remain. Again and again has the surface of our earth been changed since then. By the denudation of continents, by the deposit of strata under seas, and by the repeated interchange of seas and continents, every trace of the primeval surface of our globe has long since been either removed or concealed.

Would this have happened with the moon? or if we are to judge by the evidence of what is, rather than by the consideration of what would have befallen, has this happened with the moon?

As regards the probable sequel of the state of things which, as we have seen, must have existed when first the moon's surface solidified, it is not easy to form an opinion. On the one hand, there are reasons for supposing that for many long ages the moon would resemble our earth in having an atmosphere and oceans, though probably the atmosphere would be far rarer than ours is now, and the oceans far more limited in extent. On the other hand, it is impossible to overlook the actual facts of the case, viz. that at present the moon has no atmosphere of appreciable density, and no ocean surface at all, while the theories which have been advanced to explain the removal of an atmosphere and oceans formerly existing are, to say the least, not altogether satisfactory. They might account perhaps for the disappearance of a very tenuous atmosphere, and the drying up (or rather the soaking in) of oceans of limited extent; but scarcely for the disappearance of all signs of an atmosphere and oceans at all resembling those of our own earth.

On the whole, I am disposed to think that those features of our moon which have been regarded as indicating the former existence of oceans—as, for instance, the darkness of the low-level regions called seas, the existence of regions looking like alluvial deposits, and so forth—may be regarded as indicating only the existence of regions which remained liquid long after the rest of the moon's surface had solidified.

I would not deny the possibility, or even the probability, that in these regions there may formerly have been considerable seas. Nay, they may possibly have been entirely sea-covered. But it certainly has not yet been proved that they ever were so.

Of course when the moon's surface was partially solid or even merely plastic and partially liquid, all the liquid matter would seek the lower levels. The plastic surface only would retain the marks of meteoric downfalls: that is, the traces of the fall of those many thousands of large masses which we have seen must have struck the moon during her plastic era. Where the liquid surfaces existed, no such traces could be retained, any more than the marks of rainfall can be retained by the surface of the sea.

On the one hand, then, if we suppose the atmosphere of the moon in remote times exceedingly tenuous and the seas very limited in extent, the effects of aerial denudation would be utterly insignificant compared with those which we recognise on the earth; so that we might expect the signs of meteoric pitting to be very little disturbed during the comparatively short era of the moon's existence as a habitable world. On the other hand, we could not expect any traces of meteoric downfall to remain in the low-lying regions to which the liquid portions of the moon's surface formerly flowed. Only when this liquid matter had either solidified or been gradually withdrawn into the moon's interior, could irregularities be formed, retained, or recognised in these regions.

If these *à priori* considerations are just, it would be found—first, that the high-level regions of the moon would be marked by multitudinous small craters of all dimensions, from the minutest which the most powerful telescope could recognise to craters a mile or two in diameter; secondly, that the low-level regions would present a different colour, and, as it were, texture, being formed of different matter which, retaining its liquidity longer, had necessarily come to form the lower lunar levels; thirdly, that comparatively few craters, and those mostly small ones, would be found over these low-lying regions. To these probable features may be added, but with less antecedent likelihood, this—that in the arrangement of the smaller lunar craters, peculiarities might sometimes be recognised indicating the occasional fall of a flight or string of meteors such as we sometimes see travelling athwart our skies even in these times when the supply of meteoric matter is all but exhausted by comparison with the wealth of meteors formerly existing.

Now let us see how these anticipations accord with the facts. To avoid all possibility of prejudice I will take the account of lunar details from a work written by an official astronomer, one therefore not likely to consider even, far less to be prejudiced in favour of, speculations respecting the past history of the heavenly bodies (any more than a land surveyor or a civil engineer would be likely to dwell upon geological speculations respecting the soils or surfaces with which he has officially to deal). I must admit that Professor Newcomb, to whom I refer, differs entirely from most European official astronomers in this respect, as do others of his countrymen. In writing his treatise on astronomy he does not seem by any means to have thought it essential to eschew all consideration of the physical significance of observed facts. I would therefore have taken a description of the moon by some one else, some official astronomer of the purely surveying order; but unfortunately the descriptions of the moon in their writings are too incomplete to be of interest or value; and any thoughts as to the moon's probable conditions, either now [1] or in the remote past or future, would be sought in vain. Let us hear, however, how Professor Newcomb describes the features of the moon which specially concern us here.

'As the moon is now seen and mapped,' he says, 'the difference between the light and dark portions is due merely to a difference in the colour of the material, much of which seems to be darker than the average of terrestrial objects. . . . Galileo saw that the brighter portions of the disc were [are] broken up with inequalities of the nature of mountains and craters, while

[1] Not long ago, a picture which some ingenious artist had painted to represent a lunar landscape, was sent to the Astronomical Society, for exhibition at one of the evening meetings. Many remarks were made on the probable accuracy or inaccuracy of various features of this fanciful but attractive painting. (In some respects it was decidedly inaccurate.) At last the chief official astronomer rose, and many expected that remarks of considerable interest would be addressed to the meeting respecting the lunar landscape. His actual speech was simply as follows: 'Mr Chairman, I move that this picture be demitted to the floor.'

the darker parts were [are] for the most part smooth and uniform. . . . It is very curious that the figures of these inequalities in the lunar surface can be closely imitated by throwing pebbles upon the surface of some smooth plastic mass, as mud or mortar. . . . There is no more real smoothness in the regions of the supposed seas than elsewhere. The inequalities are smaller and harder to see on account of the darkness of colour, but that is all.'

As to peculiarities of arrangement, Webb remarks on the tendency to parallel direction among craters, and local repetitions: 'Two similar craters often lie north and south of each other, and near them is frequently a corresponding duplicate. Two large craters occasionally lie north and south, of greatly resembling character, the southern usually three-fourths of the northern in size, from eighteen to thirty-six miles apart, and connected by ridges pointing in a south-west direction. Several of these arrangements are the more remarkable, as we know of nothing similar on the earth.'

If the views above considered are just—and it seems to me very difficult to controvert them—the multitudinous small craters would be due to external action, and they would be earlier formations in the main than the larger craters due to the reaction of the moon's interior upon the contracting crust. Thus we might expect to find regions covered with small craters affected by the results of contractive processes and internal resistance to such contraction, in such sort that all the small craters would be distorted and all similarly. Beer and Mädler describe a lunar feature corresponding with what we should thus expect, speaking of 'small craters entangled in general pressures, and squeezed into an oval form,' the effect being 'like that of an oblique strain upon the pattern of a loosely-woven fabric.'

It will be understood that I do not consider the larger features of the moon as necessarily or probably due to external action. I cannot see how the crust of the moon while plastic can have escaped being marked by multitudes of small craters; and I do not think it likely that the pitting thus caused would be obliterated by subsequent processes of denudation. Thus I regard the crowded small craters which exist on the higher regions of the moon's surface as most probably due to meteoric downfall. But the crust thus pitted externally would, during later stages (or possibly contemporary stages) of the moon's progress, undergo changes resembling those which have affected our earth's crust.

First, the crust contracting more rapidly than the nucleus, because parting more rapidly with its heat, would be exposed to tremendous strains, corresponding precisely with those which would result from the expansion of a nucleus within an unchanging shell. It would probably be to this stage of the moon's development that we must refer the systems of radiating streaks which form so marked a feature of the lunar globe.

Secondly, the crust having cooled with comparative rapidity (though millions of years were probably required for this process), the nucleus would in its turn begin to cool more quickly than the crust, having more heat to part with. Accordingly, spaces would form between the nucleus and the crust, were it not that the action of gravity would compel the crust to follow up the contracting nucleus. From this process two things would follow: first, massive corrugations would form on the surface of the moon; in other words, mountain ranges and all orders of ridge-shaped irregularities; secondly, the heat resulting from this mechanical process would, as in the case of our own earth even to this day, cause volcanic explosions, and result in the formation of mighty craters.

But with these stages of the moon's development I am not at present concerned. It is with the multitudinous small craters which cover all the higher regions of the moon that I have sought to deal. It appears to me that whether we consider what must have happened as the moon passed through the plastic and semi-plastic stages of her existence, or whether we consider the evidence derived from the actual condition of the moon's surface, we are alike led to the conclusion that the innumerable small craters which cover the higher lunar levels have been caused chiefly by meteoric downfall. When I first advanced this theory (in 1873) I had not yet fully recognised the evidence both *à priori* and *à posteriori* in its favour. I said then that 'I should certainly not care to *maintain* it as the true theory of the origin of the small craters,' though I pointed out that 'as yet no plausible theory has been urged respecting this remarkable feature of the moon's surface.' I now view the subject differently. The evidence in favour of the meteoric theory of the small craters is much stronger than I at first supposed, the difficulty of forming any other plausible theory much greater. I may even go so far as to say that it would be a problem of extreme difficulty to show how a body formed like the moon, exposed to similar conditions, and for the same enormous time-intervals, could fail to show such markings as actually exist on the moon. A theory of which this can be said stands on a somewhat strong basis. But, after all, I believe no amount of abstract reasoning will do so much to indicate the probability of this explanation as a brief study of the moon's surface with a good (not necessarily a very powerful) telescope. If this essay should lead some thus to examine the moon who have never yet done so, not only will it have subserved a useful purpose, but the pleasure they will derive from the novel experience will be deemed, I am satisfied, a sufficient reward for whatever time and attention they may have given to the reading of this paper on the smaller craters of our satellite.

A NEW CRATER IN THE MOON.

Many astronomers appear to regard with something like contempt inquiries into the physical condition of the heavenly bodies. The movements of the celestial mechanism alone have interest for them, while the study of the present condition of the sun, moon, and planets, of their probable past and of their probable future, is regarded beneath the dignity of the astronomer. This, however, has never been the feeling of the true masters of the science. In old times, when men were utterly unable to obtain information respecting the physical condition of the planets, astronomers were obliged to content themselves with the study of the mere movements, real or apparent, of the heavenly bodies. But from the time when the discovery of the telescope enabled astronomers to study the planets as from nearer points of view, all the greatest astronomers, Galileo, Huyghens, Newton, the Herschels, and others, have taken as deep an interest in questions relating to the condition of the heavenly bodies as in the mathematical investigation of their motions. There have been, and there are still, astronomers who are mere mathematicians, just as there have been and are still mathematicians who are mere calculating machines. Nay, it must in justice be said that some of the most important astronomical discoveries of the age have been due to one-sided astronomers of this kind. But it is impossible to class with the great men who have made astronomy what it is, those mathematicians, however skilful, who, unlike Newton and Laplace (the greatest mathematicians as well as the greatest astronomers), seem only to have valued the study of astronomy because of its fruitfulness in mathematical problems.

There are few departments of astronomical research in which the distinction above-mentioned is more characteristically presented than in the study of our moon. To the merely mathematical astronomer, the moon simply presents a highly interesting subject for mathematical computations, and for instrumental observations intended to check such computations, or to suggest fresh matter for calculation. They regard the discovery of some minute discrepancy between the observed and calculated motions of the moon, as infinitely more important than the recognition of any signs of physical change in the moon's surface could possibly be. I believe that if the moon's globe were (through the action of some quite inaccessible forces) to burst into fragments, the catastrophe would be looked upon by astronomers of this class as only interesting because affording an illustration of the dynamical effects of explosion, and suggesting divers matters for calculation.[1] The recognition of the signs of life upon the

[1] A few days after the recent transit of Mercury I met one of the most eminent of our mathematical astronomers, and expressed disappointment at the unfavourable weather which had prevailed in England during the transit. It had seemed to me a matter of considerable interest to determine whether Mercury has an atmosphere, and if so, of what nature that atmosphere may be, to

moon, if we could imagine the possibility of life on that sterile surface, would probably have no interest at all for the merely mathematical astronomer; for it would scarcely be possible to connect the discovery with d^2y by dx^2. I would not be misunderstood as wishing to imply that there is little interest in the mathematics of astronomy. On the contrary, I not only recognise great interest in the mathematical relations to which I have referred, but I have a strong feeling of sympathy with those who are specially attracted to that particular department of astronomical research. There is a wonderful charm about mathematical astronomy, as, indeed, about mathematics generally; and there is exceeding interest in many of the physical discoveries which have resulted from mathematical calculations. It would, indeed, be a mistake of the same kind as that which I decry to depreciate the interest of such discoveries. For what I would insist upon is, that the true astronomer should regard with interest the results of all forms of astronomical research. And although the recognition of some new feature of the moon's surface may not have involved any profound mental effort, or the exercise of exceptional skill in mathematical calculation or instrumental observation, yet to all who rightly appreciate

examine into the phenomenon of the bright spot said to have been seen on Mercury, and so forth; and to make better inquiries into the physical condition of Mercury. But I found all such inquiries were regarded by him as utterly trivial—the only point he cared to speak about being a discrepancy of a few seconds in the moment of contact. Yet, oddly enough, he had published a formula for predicting the time of contact, which I found far from exact.

the object of scientific research must admit the great interest of the questions suggested by supposed lunar changes.

No further apology need be made, therefore, I think, for a brief inquiry into the subject of the new lunar crater said to have been discovered by Dr. Klein, of Koeln.

It may be well first to consider the *à priori* probabilities of changes in the moon, such as might be detected from the earth; but briefly, because otherwise space would hardly suffice for the due consideration of supposed instances of change. It has always seemed to me that such *à priori* consideration of a subject is a desirable preparation for the examination of *à posteriori* evidence, so long only as we are careful not to allow the consideration of what might be expected to prevent us from duly attending to what has actually been observed.

Without dwelling upon the earlier stages of the moon's history, we may fairly assume that the moon was once an intensely heated globe, and that she has passed through many stages of cooling to a far later stage of planetary life than that through which our earth is at present passing. Nearly all who have ever investigated the evidence afforded by the moon's telescopic aspect agree in this conclusion, though in other respects they entertain widely discordant opinions.

We may recognise three special stages in the moon's cooling which correspond with stages through which our earth has already passed. First there was the stage in which a lunar crust and a lunar nucleus were formed (observe, that I do not here adopt any theory as to the nature of either; I infer only from what we know about our earth that at a very early stage of planetary cooling the nuclear regions and the inclosing shell of matter became distinguished one from the other, in such sort that thereafter each obeyed a distinct set of influences corresponding with its position and with the conditions to which it was exposed). Secondly came the stage in which the exterior shell, cooling more rapidly than the nuclear matter, contracted upon the nucleus—a process leading to the formation of rifts and clefts in the crust, precisely as though the nucleus had expanded within the inclosing shell. Thirdly, when the crust had thus parted with the greater portion of its heat, there came the stage when the nuclear matter, now far hotter than the crust, cooled more quickly (having more heat to part with), and thus shrank away from the crust.

In the case of our own earth, it was during the second of these great stages, which lasted probably for many millions of years, that the great deformations of the terrestrial spheroid had their origin. In the third stage were formed those corrugations of the thickened crust, constituting the various orders of mountain ranges. To the latter part of this third stage belong the forms of volcanic activity which still exist upon our own earth, and may, perchance, exist to some degree in the moon. At any rate, if the supposed changes in the moon's surface are to be attributed to vulcanian activity, such activity can only be regarded as belonging to the latest era of the third and last stage of planetary evolving. The question, in fine, which we have to determine, in considering these changes, is simply this: Are they vulcanian (using the word as Mallet does), or are they to be otherwise explained? Or we may put the question thus: 'Is the moon's frame dead, or does it still retain the last sparks of planetary life?'

Now, there is one question underlying all our inquiries into the moon's actual condition as revealed under telescopic scrutiny, which appears to me very difficult indeed to answer. It *has* been answered, by some, as though it were very readily dealt with. Unfortunately, it has been answered in several different ways. The question is this: Has the moon once had an atmosphere and oceans like the earth? It will be manifest that if we suppose the moon to have resembled the earth in this respect, at a remote epoch, and during a long period of time, we should give to observed appearances on the moon's surface an interpretation very different from that which we should give if we supposed that the moon's globe never had much water upon it, and was never enveloped by an atmosphere of considerable density. In one case we should have to take into account the influence of long-continued denudation; in the other, the only effects we should have to consider would be those resulting from vulcanian energy and those depending on the alternation of intense heat and intense cold during the long lunar day of twenty-nine and a half terrestrial days.

Assuming that the moon was ever clothed with an atmosphere, and partially covered by oceans, we could form, as I have elsewhere shown, but one opinion as to the way in which the oceans, at any rate, have been caused to disappear. We should be compelled to believe that the water had been withdrawn into the moon's interior as the moon cooled. The disappearance of the atmosphere could hardly be explained in the same way, however. For it must be remembered that a lunar atmosphere resembling our own in density, at the former lunar sea-level, or even an atmosphere of only one-tenth or one-hundredth the density of ours, would extend much farther above the moon's surface than our atmosphere extends above the surface of the earth. At a height of about three and a half miles our atmosphere is only half as dense as at the sea-level, at seven miles one-fourth, at fourteen miles one-sixteenth, at twenty-eight miles one-256th, at forty-two miles one-4096th. But whatever the density of the lunar atmosphere at the surface of the moon, it would only be reduced to one-half at a height of twenty-two miles, to one-fourth at a height of forty-four miles. So that, if at the surface its density were only one-thousandth that of the air we breathe, the density at a height of forty-four miles would be somewhat greater than the density of our own air forty-two miles above the sea-level; and at all greater heights the density of the lunar air would be enormously greater than that of our own air at corresponding heights. (Indeed, it is worthy of notice, that even assuming the lunar air as rare as it is commonly supposed to be, the density at great heights is greater than that of our own air at the same height). Seeing then that the lunar air is so much less closely packed, so to speak, by the action of gravity, than our own, we cannot suppose that an atmosphere at all resembling ours in density has been withdrawn into the moon's interior. On the other hand, it is difficult to believe that such an atmosphere has entered into chemical combination with various substances forming the moon's crust, and has thus disappeared as an atmospheric envelope.

On the whole, it seems to me that the balance of probability is strongly against the former existence of a lunar atmosphere resembling our own in density. If this be assumed, then we should no longer have to account for the effects of what geologists call sub-aerial denudation, upon the moon. The moon's surface would show only the effects of past forms of vulcanian activity. It must be admitted, I think, by all who have ever studied the moon with the telescope, that the aspect of the lunar mountains, craters, plains, &c., accords better with this view than with the other. It is almost impossible to believe that the earth will ever present a scene at all resembling that now presented by the moon, simply because we see that even if the waters of the sea were withdrawn from the earth, and all forms of life, animal and vegetable, disappeared, the effects of the long ages when the earth had air and water would remain, and would perceptibly modify the earth's aspect. There is very little on the moon's surface, as at present seen, which can possibly be attributed to such a cause.[1] Thus the probability is increased that the moon never had an atmosphere of considerable density, even if she ever had widely extended oceans.

The importance of this point will be seen when it is remembered that if the moon ever had oceans, and an atmosphere such as the earth has, it is certain that the moon must have reached a condition of extreme planetary old age, since otherwise the oceans and atmosphere could not have disappeared so completely as they have; whereas, on the other hand, if the moon never had widely extended oceans, or an atmosphere of considerable density, we need not necessarily assume that all vulcanian activity has disappeared.

If then it should appear that craters of considerable size may be formed in the moon's crust even now, it would follow of necessity (1) that the moon is not utterly decrepit, and (2) that she can never have had oceans of great extent or an atmosphere of considerable density. It will be seen that I consider the *à priori* evidence as somewhat favouring the second of these conclusions, though not therefore the first. For it by

[1] I am not sure but that even those few features which have been attributed to the former existence of water on the moon, as the colour of the lower lunar levels, the supposed signs of glacial action, &c., may not all be equally well explained by referring them to the time when the moon's crust was first found, the lower levels representing the regions occupied by the portions of the surface which last remained fluid.

no means follows that because, if the moon is not now in the latest stage of planetary life, she can never have had a deep atmosphere and large oceans, therefore if she never had a deep atmosphere and large oceans she cannot now be in the latest stage of planetary existence. A thousand millions of years hence the moon will probably present much the same appearance as at present, although, supposing her not now in her final stage, she would be in her final stage then. On the whole, while it seems to me on *à priori* grounds exceedingly probable that very little water and only air of extreme tenuity ever existed on the moon, it appears to me even more probable (speaking always of *à priori* evidence only) that the moon has reached the latest stage of planetary life.

Nevertheless it must be admitted that these questions can only be satisfactorily resolved by *à posteriori* evidence. The considerations adduced may lead us to look with somewhat more caution on observed evidence of change than we should do if antecedent probabilities led us to expect change; but if we should find unmistakable evidence of change, we must conclude that the opinion we had based on antecedent probabilities was incorrect.

The first case to be considered is that of the lunar crater Linnæus or Linné. For two reasons this case is more satisfactory, as will presently appear, than that of the new crater supposed to have recently appeared.

In the lunar plain, called the Sea of Serenity (probably because we have no reason to suppose it is exceptionally serene, while we are certain it is not a sea), there was once a deep crater, about 6½ miles across. It was very distinct when the sun's rays fell obliquely on it—that is, shortly after the time of sunrise and shortly before the time of sunset there. But when fully illuminated the crater was not a well-defined object; the tint of its floor is indeed markedly lighter than that of the surrounding plain, but the light tint merges gradually into that of the Sea of Serenity. Thus Mädler; but Lohrmann described the crater as only about 4½ miles in diameter. Both these observers agree in describing the crater as deep, and having steep walls. Now in November 1866, Schmidt of Athens announced that this crater was missing. To understand the importance of this announcement, if the crater originally existing had been filled up, let it be noted simply that the quantity of matter necessary to fill that crater would be at least equal to that which would be required to form a mountain covering the whole area of London, to a height of two miles! Naturally astronomers were greatly interested by Schmidt's discovery, and during the years 1867 and 1868 many observations of Linné were made with telescopes of great power.

The result of these observations was to show first that the area originally occupied by the crater and its outer slopes still presented a whitish aspect under the illumination of a high sun. In extent, then, the region of the crater had not changed. Secondly, within the white region a shallow circular depression, about seven miles in diameter, was recognised, with sloping sides (on the inside as well as the outside), so that at the bottom the depression had a diameter of only about three miles, the depth of the depression being about a third of a mile. Thirdly, within this shallow depression a small crater about half a mile in diameter on the inside, and of considerable but unknown depth, was detected.

There could be no doubt whatever that a great and important change had taken place in Linné—a change compared with which the most tremendous volcanic action on our own earth within historic times would be almost as nothing—were it not that some old drawings by Schröter seem to present the crater much as it now appears. It has, however, been generally recognised that more reliance can be placed on Lohrmann's and on Mädler's drawings than on Schröter's. If Lohrmann and Mädler had not definitely and independently described Linné, we might hesitate to accept their drawings, and prefer Schröter's simply as agreeing better with the present aspect of the crater. But it seems difficult to reject their concurrent testimony, while Schröter's drawings have never had much weight with lunarians, or selenographers as they prefer to be called. It is noteworthy, however, that amongst Schröter's observations of Linné is one recording that in November 1788 the crater was occupied by a dark spot, instead of appearing as usual somewhat brighter than the neighbouring regions.

Are we then to believe that a crater some seven miles across, and two or three miles deep (nothing less would correspond with Lohrmann's and Mädler's description, 'very deep'), had become in some way filled up to within a third of a mile of its lips? Sir John Herschel not only accepted this stupendous idea, but even went somewhat farther. 'The most plausible conjecture,' he said, 'as to the cause of this disappearance, seems to be the filling up of the crater from beneath, by an effusion of viscous lava, which, overflowing the rim on all sides, may have so flowed down the outer slope as to efface its ruggedness, and convert it into a gradual declivity, casting no stray shadows.' This indeed was my own notion at the time when Schmidt's announcement was first made, and the present aspect of Linné determined. In an article which appeared in the 'Temple Bar Magazine' in 1867, I even went so far as to say that this was the only explanation available, viz. that a mass of matter had been poured into the crater from below, and had overflowed the barrier formed by the ring-mountain, so as to cover the steep outer sides of the ring,

Since that time, however, I have had occasion to study closely a number of considerations which I did not then take duly into account. Scarcely any amount of evidence would in my opinion establish the existence of internal forces so tremendous as would be implied by the theory I advanced in 1867. Indeed, if it could be proved that the reaction of the moon's interior against her crust is capable of producing such effects as these, it is quite certain that elsewhere much more obvious effects would be exhibited. A crater seven miles in diameter and very deep, so situated above a region of vulcanian activity as to receive into its mighty basin sufficient lava to fill it, and overflowing its sides to obliterate all traces of the former high walls, would be a safety-valve for such a region. Ordinarily it could not happen that internal forces so tremendous would find so ready an outlet. We should see regions much larger than Linné completely riven and devastated by the action of the moon's internal energies. Unless indeed we supposed that at two or three spots only these tremendous energies are at work, while elsewhere there is comparatively little disturbance; but such a supposition is manifestly opposed to all reasonable probability.

Another explanation, which in 1867 I regarded as less probable, as rather less accordant with observed facts than the theory enunciated above—subsequently adopted by Sir J. Herschel—seems to me now the only admissible explanation of the change in Linné. The ring-shaped wall around the deep crater had not, I then thought, been destroyed, because if it had, its fragments and their shadows would remain visible. But I overlooked two things—first, the possibility that the fragments of the destroyed wall would be too small to be separately discernible; and secondly, the probability that the downfall of the wall would be accompanied by the development (for a short time) of an intense heat, competent, if not to liquefy, yet to render plastic, the matter which had before formed the base of the wall, in such sort that the fragments (themselves heated by their downfall) would be more or less completely imbedded. Either explanation would suffice to remove the difficulty which I pointed out in 1867.

But it may be asked whether the downfall of a lofty and doubtless massive wall originally surrounding the crater would not itself be indicative of the action of tremendous forces of upheaval. It appears to me that the opinion we are to form on this point would depend considerably on our estimate of the condition of the lunar crust. If this crust were supposed to be very thick or to rest immediately upon the lunar nucleus, then we could hardly imagine that any save some very energetic cause could effect the destruction of a wall so large as that which formerly surrounded Linné. But if in the long processes of cooling and contraction to which both crust and nucleus have been exposed large open spaces have been formed between the nucleus and the crust, in certain regions, we could readily understand that in some cases very moderate vulcanian forces would suffice to overthrow very large masses. Now we know that the moon's mean density is much less than that of our own earth, being only about $3\frac{1}{2}$ times, whereas the earth's is nearly $5\frac{3}{4}$ times the density of water. We may fairly conclude that the crust of the moon is far less compact than that of the earth. Again, it manifestly would require a much smaller force to effect the overthrow of a steep wall of great height, than to produce an up-flow of matter equal even to but one-hundredth part of the wall in mass. Mere shrinkage of the nucleus would account for effects of the former; whereas to produce effects of the latter kind, energetic expansion, and therefore intense heat, would be required.

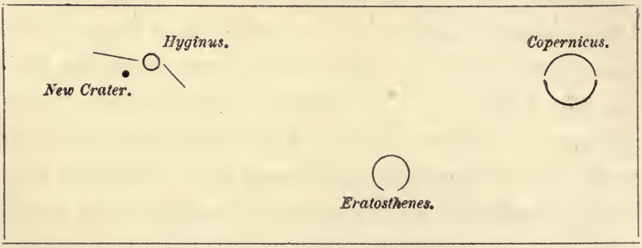
Apart, however, from any vulcanian forces, forces are known to be at work on the moon, which might fairly account for the overthrow of steep and lofty walls. During the intense heat of the long lunar day, a process of expansion takes place, which must affect the moon's crust to a depth of several hundred yards. Different parts of the moon's surface must be differently affected according to their substance, colour, contour, and so forth. After day has passed comes the long lunar night, in which intense cold affects the moon's surface. The change from a heat surpassing that of boiling water to a degree of cold far exceeding that of our bitterest arctic winters, cannot but produce a steady disintegration of the lunar crust. I observe, indeed, that Professor Newcomb, in his excellent 'Popular Astronomy,' expresses the opinion that these changes of temperature are not sufficient of themselves to produce any effect, though they powerfully re-enforce the action of causes due to the existence of air, water, &c. on a planet. Since on the moon, he says, there is 'neither air, water, rain, frost, nor organic matter, the causes of disintegration and decay are all absent. A marble building erected on the surface of the moon would remain century after century just as it was left. It is true that there might be bodies so friable that the expansions and contractions due to the great changes of temperature to which the moon is exposed would cause them to crumble. But whatever crumbling might thus be caused would soon be done with, and then no further change would occur.' This view of the matter is, however, altogether untenable; expansions and contractions in masses of different substance cannot take place without some degree of friction; and friction long continued will, however gradually, destroy the strongest material. It is only a question of time.

Nevertheless, it must be admitted, I think, that if Linné really has undergone the tremendous change in which many astronomers believe, some degree of vulcanian energy must have been at work. The mere expansion and contraction of the crust could scarcely have brought about the downfall of a ringed wall nearly seven miles in diameter at once, or (at the outside) in a few weeks. The first impulse towards the process of destruction may have been given in this way; but sublunarian forces must have helped to produce so complete an overthrow of the great wall which seems to have existed when Lohrmann and Mädler made their (independent) observations.

Let us turn now to the change which is supposed to have more recently taken place on the moon.

We have to direct our attention to a spot lying near the middle of that face which the moon turns constantly towards the earth. Linné, though not near the edge of the lunar disc, is yet far enough removed from the centre to be considerably affected by those apparent swayings to and fro of the moon's globe which are called her librations. As I have shown in my treatise on the moon (not in the first edition, but in the second), any irregular surface lying at a considerable distance from the centre of the moon's face may be very much changed in aspect in consequence of these librations, whereas a region near the centre, though it may be to some degree affected, must be much less changed than a similar region near the edge.

Not to trouble the reader with a long description of lunar topography, let me thus indicate, sufficiently for my present purpose, the position of the crater which is supposed by many to have recently formed in the lunar plain called the Sea of Vapours.



Every student of the moon knows the craters Copernicus and Eratosthenes. Copernicus lies on the tip of the imaginary nose of the 'man in the moon' (that is, of the 'face' imagined in the moon, not of the imagined figure of a man with bundle of sticks and dog), and has been compared to a mighty carbuncle there. Eratosthenes lies a little higher on the ridge of that feature. In the above map, the region shown is inverted, to correspond to what the telescopist sees; it is only necessary to hold the page upside down[1] to

[1] Sometimes erroneous directions are given for this purpose; as

see the craters in the position they actually occupy on the moon's face. Hyginus is a much smaller crater than either Copernicus or Eratosthenes, but is equally well known to students of the moon on account of the great rift which passes through the crater, extending outside in the directions shown by the straight lines in the little map.

When examining this part of the moon's surface with a $5\frac{1}{2}$-inch telescope, on May 27, 1877, Dr. Hermann J. Klein, of Köln, observed a crater in the position shown by the small black dot in the above map. At the time of observation, the moon had passed her third quarter by rather more than half a day, and the floor of the crater was in shadow. Thus it appeared black. It seemed to be nearly as large as Hyginus, or nearly three miles in diameter. Klein describes it as deep and full of shadow, and forming a conspicuous object on the deep grey Sea of Vapours. 'Having frequently observed the region during the last few years, Dr. Klein felt certain that no such crater existed in this region at the time of his previous observations.' He communicated his observations to Dr. Schmidt, of Athens, who assured him that this crater was absent from all his numerous drawings of this part of the lunar surface. It is not shown in the maps made by Lohrmann and by Beer and Mädler, nor does Schröter or any of the older lunarians indicate a crater at this part of the moon's surface.

to invert the picture and also look at it behind or at its reflection in a mirror. But it is in reality sufficient to invert the picture. This at the same time alters it right and left.

Further observations showed that the crater either has no wall, or a very low one. It appears, in fact, to be only a deep conical opening in the surface. Soon after the sun has risen it takes the appearance of a dark grey spot, with an ill-defined edge. Later still, it assumes the same general tint as the Sea of Vapours, and can no longer be distinguished.

It will be obvious that this case is not, at a first view, so striking as that of the crater Linné. It is proverbially difficult to prove a negative. In the case of Linné, a crater which had not only been marked in maps by different observers, but had been definitely described by them as very deep, was found to be, either missing altogether, or at least very shallow. If it had not been for the doubts suggested by Schröter's observations, it would have seemed as though nothing could be clearer than the proof of change in such a case as that. On the other hand, in the case of the crater observed by Klein, before the occurrence of change can be regarded as proved, we must have decisive evidence that the crater did not exist before. The only evidence we have is, that it had never been seen before. Now, the knowledge that an object has not been seen, may, under certain conditions, amount almost to moral certainty that it did not exist; and what we have to determine, in the present instance, is the weight of the argument from probability, based on the failure of Schröter, Lohrmann, Beer, Mädler, Schmidt, and others, to recognise the crater during their multitudinous observations of the moon. It will be understood, of course, that we have not the direct evidence of any one of these observers in favour of the non-existence of the crater before the year 1877. Not one of them has recorded that, having carefully searched the region inclosed between the two branches of the great rift through Hyginus, they ascertained that there was no crater there exceeding—let us say—half a mile in diameter (smaller craters not being recognisable with the telescopes they used). If they had, the negative evidence would be as satisfactory and decisive as the best positive evidence. All we know is, that in none of their observations did they take notice of a crater in that particular position.

Unquestionably it would be very remarkable if all these observers had omitted to notice a crater really existing where one now assuredly exists. It is true craters so small as this one are exceedingly numerous in the moon. And the chance of any crater escaping notice on any particular occasion is no doubt much greater than those unfamiliar with the study of the moon might opine. Many seem to think that on any night in the year the astronomer can study any crater on the moon except on one night per lunar month, when the moon is 'new.' But in reality the occasions when a crater can be well seen are not by any means numerous. Take, for instance, the 'new' crater, if so it must be considered. Until the moon is nearly six days old this crater is in darkness. About that time the sun's rays begin to fall on the region round the crater, causing the interior of the crater to appear as a minute dark spot. A day or so later, the darkness of the spot is notably reduced because the light begins to mark the floor of the crater. When the moon is seven and a half days old a greyish spot with diffused edge is seen. When the moon is about nine days old the grey spot can hardly be detected, and a few hours later no trace of the crater can be perceived.

But it may seem to the reader who may chance not to be familiar with astronomical relations, that if the spot is visible for three days about the time of first quarter, and for three days also about the time of third quarter, that allows the lunarian some six days per lunar month in which the new crater might be seen. This should be ample time to study the moon so closely over the region near Hyginus that the new crater could not possibly escape detection—at least not when such observations were continued for many years.

The opportunities, however, are not quite so favourable as might thus be supposed. The moon at her first quarter follows the sun by about six hours. As the sun is highest at noon, when due south, so the moon at her first quarter is highest, and therefore most suitably placed for observation, at six o'clock in the evening (when she is due south). If the sun has not set at this hour, in other words during the six summer months, the moon will not be so well seen as if the sun were below the horizon; for a veil of illuminated air will be spread over her face. Again, for about two months before and two months after the autumnal equinox, the moon at her first quarter attains but a low elevation when due south, precisely as the sun is low at noon for two months before and after midwinter. Accordingly it is only from November to May that the moon can be well studied near her first quarter, supposing the weather clear at or about six in the evening, when she is at her highest above the horizon. As there are thus only some eighteen or twenty days in the year on which, for about an hour, on the average, the moon can thus be favourably studied, and the like number from July to February when the moon at her third quarter can be so studied,[1] we see that the opportunities for detecting any particular small crater are not so numerous as at first sight they might be supposed to be.

Nevertheless, even when these considerations are fully taken into account, and also the circumstance that when the moon is favourably situated and illuminated for the observation of Hyginus and its neighbourhood, many other objects of interest would be apt to attract attention, it cannot but be regarded as a remarkable circumstance that a crater so easily seen as Klein's should so long have escaped detection if it really existed. The improbability of this happening is so great as to render extremely probable the inference (which a writer has somewhat rashly described as 'absolutely certain'), that previous to 1876 there did not exist on this portion of the lunar surface a deep black crater three miles in diameter. 'If, therefore,' the same

[1] The study of the moon at her first and third quarters need not absolutely cease even at Midsummer, despite the veiling of her face when due south by sunlit air.

writer continues, 'the existence of Dr. Klein's new crater be confirmed, it will form the strongest possible evidence of a real change on the surface of the moon, a change moreover of a volcanic nature.'

Unquestionably if the crater did not exist or was not visible before 1876, and can now be seen (as certainly is the case), a change must have taken place on the surface of the moon. It would remain very doubtful, however, whether the change was of a volcanic nature. I mentioned some time ago that the case of Linné affords, in two noteworthy respects, more satisfactory evidence of volcanic change than the case of the supposed new crater. One reason I have already mentioned, the fact, namely, that in Linné's case we have positive evidence, whereas in the case of the new crater we only have negative evidence. The other reason is this. In Linné's case the change supposed to have taken place can hardly be ascribed to any cause except some form of volcanic energy; in the case of the supposed new crater, volcanic action is not at all necessarily indicated. That a lofty wall circling a circular region six or seven miles in diameter should be thrown down (and so completely as to leave only a ring of fragments separately undiscernible) by the effects of mere expansion and contraction due to the sun's action, may well be regarded as altogether improbable. But there is nothing improbable in the breaking up of a floor covering a cone-shaped opening some two miles or so in interior diameter. If in some exceedingly remote era of the moon's volcanic history, the cone-shaped cavity had been filled with lava through the funnel-shaped opening at the bottom, the lower portion of the lava remaining liquid longer than the upper would eventually flow out again through the opening by which it had entered the cavity. Such a process not unfrequently occurs in terrestrial volcanoes; in fact, it probably occurs in the majority of cases. The upper portion of the lava remains, in such a case, as a thick but not very strong or massive roof spanning over the space left vacant by the lava which has retreated. The roof might break up before long, under fresh volcanic action; or it might not only resist such action for countless ages, but remain for millions of years without yielding either to the various strains and pressures to which it would be exposed as the surrounding slopes and its own mass gradually contracted with the cooling of the moon, or to the effects of the alternate expansions and contractions resulting from the heat of the lunar day and the cold of the lunar night. But we can readily understand that in the long run a floor of this kind must give way, and when it gave way it would break off altogether, the fragments falling to the bottom of the cone-shaped cavity, and some of them even falling through the funnel-shaped entrance of the cavity into the open space between the crust and the nucleus there.

But here a difficulty will present itself to everyone who is at all familiar with the aspect of the moon's surface in the telescope. If there had always been a crater where one now certainly exists, and this crater had been filled, or nearly so, with lava which had solidified and formed a floor, some difference of tint would have been recognisable where this lava floor existed. In full moonlight perhaps the place of the crater might not be discernible; but under a moderately high sun the lava should have been distinguishable from the surrounding surface.

Now here the photographs of the moon obtained during the last thirty years or so may serve us in good stead. It is true the crater is rather a small one for photography to deal with at present. Yet many objects not more than two miles in diameter are shown in some of the fine photographs taken by Rutherfurd, De la Rue, and Draper. Traces of Klein's crater might fairly be looked for in some of these views of the moon. I say in some of them, not specifying those taken nearly at the time of the moon's first and third quarter; though, of course, it would be in these that the crater as it exists now might be expected to appear, if it is not a new formation. If, however, the crater floor has recently broken up in the way suggested above, the signs of the former existence of a spot of different lustre and contour (of different texture, as it were) might be found as probably under full illumination as under a low sun.

Through the kindness of Dr. Louis Rutherfurd, of New York, I possess a fine series of twelve photographic views of the moon in various phases, besides one magnificent copy of the photograph taken on March 6, 1865. I turned to the examination of these views with considerable interest. Passing over three representing the moon before sunlight had reached Hyginus, I take next (though not next in phase) the view which shows Hyginus under the lowest illumination: viz. one taken two days or so before third quarter, on September 16, 1870. In this view (which appears in my treatise on the moon) I find a small dark spot almost exactly where Klein's crater should be; but I am inclined to doubt whether this dark spot is not an accidental mark, due to some small dust flaw in the negative. Many such dust marks are found in the same photograph, most of them being quite obviously distinguishable from the dark round shadows representing the small craters. But Hyginus is too near the edge where illumination terminates (technically called the termination) for an opinion to be formed on this point—at least, from the positive in my possession. I hope soon to hear that Dr. Rutherfurd has caused the negative to be examined. Next comes the splendid view taken on March 6, 1865, when the moon was about nine days five hours old, supposing Rutherfurd to have photographed her when nearly south at New York. Now in this view (of which, be it noticed, I have two copies, one belonging to the series above named, the other greatly enlarged) I find the place of the new crater occupied by a small space of lighter texture than that of the Sea of Vapours. It is well seen in both copies. Doubtless the negative, if carefully examined, would show more details, and possibly resolve very definitely the question whether Klein's crater is a new one at all. But certainly, whether a real change has taken place or not, the crater, with its lava interior, existed as far back as 1865. Since this covers the entire period during which Klein states that he has frequently observed this region without discovering the crater, there can be no reason for believing any change had occurred in the crater before 1865.

In a photograph taken on February 28, 1871 (forming the frontispiece of my 'Moon'), when the moon was nearly half a day older, the region occupied by the crater is lighter than the surrounding sea, but s less definitely indicated, larger and more diffused. In a splendid view, taken April 19, 1872, when the moon was a day older, the light region can be clearly recognised. In a view of the full moon taken June 2, 1871, I can detect no trace of the crater.

I note also that in a view of the moon near her first quarter, taken by Mr. Ellery with the great reflection of the Melbourne Observatory, the difference of tint of the region occupied by the supposed new crater can be most distinctly recognised.

It is hardly necessary to point out, perhaps, that the recognition of a decided difference of colour in this part of the so-called Sea of Vapours, where none of the lunar map-makers have indicated any peculiarity of the kind, tends to throw some degree of doubt on the negative evidence which can alone be adduced in favour of a change of any sort. If the light-tinted spot which has certainly existed since 1865, and doubtless for ages, escaped the scrutiny of lunarians, so also might a dark spot such as is seen when the crater is under low solar illumination. The light-tinted spot is not a mere photographic feature, so to distinguish peculiarities of tint which are shown in a photograph yet escape ordinary visual observation. The feature exists still. It can be recognised when the moon is ten days old, though it is not so obvious to the eye as in the photograph. I find, however, that even those observers whose attention has been specially directed to the supposed new crater have failed to recognise the light spot which comes into view a day or so after the crater has assumed the same tint as the surrounding plain. Thus it seems quite possible that the crater may have been always in its present condition.

But, in any case, there has probably been no volcanic change. Here, if a change has taken place at all, the floor of a crater two miles across, after undergoing for millions of years the expansion resulting from the tremendous heat of lunar midday (with a sun nearly vertical) and the contraction resulting from the fearful cold of lunar midnight, has at length yielded to lunar gravity, falling in fragments upon the sloping walls of the hollow space beneath. That changes such as this should from time to time—though probably at enormously long intervals—take place in the moon's crust, is to be expected. So far from regarding them as incredible or even surprising, we should perhaps consider the real wonder to be that they are not more frequently recognised in a surface exposed to such amazing vicissitudes of heat and cold. Whether the recognition of the downfall of a wall here or of a crater floor there is an adequate reward for the labours devoted to lunar phenomena by men like Schröter, Lohrmann, Mädler and Schmidt, may be a question with many—precisely as many fail to understand how a coleopterist can devote the best part of his life to determine the genealogy of Pediculus Melitta. But we can only hope to learn from labours such as these the actual condition of our companion planet; and perhaps we may even thus learn something of the changes which will affect our own earth millions of years hence—

> When the old hulk we tread shall be a wreck,
> A slag, a cinder,—drifting through the sky,
> Without its crew of fools.

A FIERY WORLD.

When the sun set on May 6, 1876, in Europe, a small black spot was visible upon his ruddy face, examined with a telescope of moderate power. This spot made its entry on the sun's disc at a few minutes past three for observers in England, and at about seven had traversed one-half of its course across the sun's face, being then very near the centre of the solar disc. Thus, as the sun set at about half-past seven on May 6, rather more than half the transit of the black spot across the sun was visible from England. The black spot was the disc of the small planet Mercury, whose movements, combined with those of the earth, brought him on May 6 directly between the earth and the sun. The phenomena of transits of Mercury, though not so interesting as those which attend the transits of the larger and nearer planet, Venus, are still well worthy of being carefully observed. Indeed, some observations which have been made on these occasions are full of interest, and remain as yet unexplained. These we do not here propose to consider, but to direct attention to such facts of interest as are known respecting the planet Mercury, the least of all the primary planets of the solar system.

In the first place, it may be well to make a few remarks on the exceptional position of the planet Mercury in the solar system. Setting aside as mythical the planet Vulcan, which some astronomers suppose to travel within the orbit of Mercury, there is no planet which is so powerfully swayed by the sun, so brilliantly illuminated, so intensely heated by his rays, as this small planet. If we could form an opinion of the importance of a world from the activity of the forces exerted upon it by the orb which dispenses light and life to our earth, we should judge Mercury to be the most favoured, and therefore the most important, of all the members of the sun's family. On the other hand, if we judged of the planet's importance from its size and mass, we should regard Mercury as relatively altogether insignificant. He resembles our moon much more nearly than he does the earth in these respects. In fact, his diameter is but half as large again as the moon's, while the diameter of the earth exceeds that of

Mercury in the proportion of more than two and a half to one. It is probable that the largest of Saturn's moons exceeds Mercury in size, though probably Mercury has the greater mass.

Since considerations so opposite in character are suggested by the simpler and more obvious characteristics of this planet, it may be worth while to examine the evidence we have a little more closely.

If we take the earth's distance from the sun at 92½ millions of miles (for the result obtained from the British Delislean observations of the transit of 1874, which would set the sun's distance at 93⅓ millions of miles, can hardly be accepted as valid), the distance at which Mercury travels from the sun has an average value of about 35,800,000 miles. But the planet's path is exceedingly eccentric, insomuch that while the greatest distance amounts to 42,100,000 miles, the least does not exceed 28,500,000 miles. In fact, it may be said that the least distance of Mercury from the sun is little more than two-thirds of his greatest distance. Thus the supply of light and heat received by him from the sun when at his least distance is greater than that received by him when at his greatest distance in nearly the proportion of 9 to 4, or is more than twice as great. This is a very important feature in the economy of the planet, regarded at any rate as possibly the abode of living creatures. The corresponding variation in our earth's case is too small to enable us to form any satisfactory idea of the effects of so marked a change. In winter the sun's distance from us is less than in summer in the proportion of about 30 to 31, and his direct heat is greater in our northern winter than in our northern summer in about the proportion of 16 to 15—a statement which may perhaps seem less perplexing (though in reality unchanged) if put in this way: During our northern summer we receive less heat than the inhabitants of the same latitudes in the southern hemisphere receive during their summer, in the proportion of about 15 to 16. We do not notice the corresponding change in the apparent size of the sun; but we should certainly do so, and we should notice also some very remarkable changes in the supply of light and heat, if his disc looked twice as large at one season of the year as at another.

Even in the case just imagined, however, we should have ample time to prepare for the change, seeing that it would require half a year, or $182\frac{3}{4}$ days, for the sun to change his face from its largest to its smallest size. Mercury, however, travels at once more quickly and in a smaller path around the central sun. In every second of time the earth sweeps over a distance of some $18\frac{1}{2}$ miles; but Mercury rushes in the same time over nearly 30 miles. The circuit of his orbit is but about two-fifths that of our earth's. Sweeping over this smaller orbit with his greater speed, Mercury completes his circuit in a little less than 88 of our days. Thus the sun passes from his fullest glory when nearest to his least when farthest in 44 of our days, or little more than six weeks.

But this is not all. Our sun might oscillate in apparent size—on which of course depends the quantity of light and heat we get from him—so widely on either side of his present mean aspect that his largest would be twice as great as his least disc; and yet we might bear the change. There have, indeed, been periods in the earth's past history, though far removed in years from the present time, when the sun's annual range of apparent size was largely greater than it is now; yet we know that the various races subsisting on the earth now are descended from creatures which bore, without being destroyed, the annual changes of temperature which must then have taken place. We might bear so great a degree of relative change even if the change from greatest to least supply of direct solar heat were completed in six weeks, as in Mercury's case. At least there is good reason for supposing that if the present order of things changed to that imagined order, as slowly as the actual changes have taken place in the past—these things requiring hundreds of thousands of years for their completion—the different races inhabiting the earth would vary accordingly. In that case, though quite possibly the creatures which would inhabit the earth when the imagined changes were effected would be unlike those now existing, their remote progenitors, yet they would not be so utterly unlike that their relationship to present races would not be recognisable. But it does not seem equally possible to admit that, even in the enormous intervals of time with which modern science has to deal in considering the life history of our earth, creatures such as we are familiar with could so change as to be able to endure the vicissitudes, or even the usual order of things prevailing in the planet Mercury. For there is not merely in that planet a rapid change from a sun pouring a certain amount of heat to a sun pouring twice as much heat directly upon the planet, but the sun, even at his mean distance, pours seven times as much heat upon each square mile of the planet's surface as upon each (similarly situated) square mile of the surface of our own earth. The actual range is from a supply of light and heat about 4½ times greater than ours to a supply almost 10½ times greater than ours. It must be admitted that the largeness in the amount of the supply would of itself suffice to render Mercury uninhabitable by any kind of animals now living on the earth, or even by any that we could imagine as the remote descendants (or progenitors, it matters not which way we take our range in time) of any known animals. When to the enormous absolute supply of heat we add the wide range of variation in the amount directly received, and the rapidity with which the amount passes from maximum to minimum and from minimum to maximum, we seem almost compelled to acknowledge that no kind of living creatures which we can even conceive could now inhabit the small world which travels nearest to the central sun.

Before, however, concluding that Mercury, if he is to be inhabited at all, can only be so when the lustre and heat of the sun have been greatly reduced—a state of things which must not be looked for until after millions of years have elapsed—let us enquire how far the intensity of the solar heat poured upon Mercury may be mitigated perhaps by atmospheric peculiarities.

At the outset of this enquiry we are perplexed to determine in what direction the planet's atmosphere should differ from our own, in order to make the vicissitudes of the Mercurial climates and the intensity of the solar heat more endurable. Should the atmosphere be rarer or denser? If rarer, we can imagine that something like an Alpine climate might prevail in Mercury, the intensity of the solar heat being mitigated by the coldness of the tenuous air; but then we have the effect of the direct rays increased in consequence of the tenuity of the air, and a contrast between heat and cold introduced which would be even more disastrous than the contrast between the intense heat when the sun is farthest and the still intenser heat when he is nearest. 'We must not deceive ourselves,' to quote words written by us some years ago, 'by inferring that mere rarity of atmosphere can compensate fully for an increased intensity of solar heat. It is not true that the climate of a place on the slopes of the Andes or Himalayas corresponds to that of a region on the plain which has an atmosphere equally warm. The circumstances are, in fact, wholly different.[1] On the plain

[1] A similar view has been taken by the ingenious and original French writer Flammarion, in a passage (pp. 149–151 of his recently published work *Les Terres du Ciel*) beginning, 'Nous ne devons pas nous tromper nous-mêmes néanmoins, en calculant que la rareté de l'atmosphère pourrait à elle seule compenser pleinement l'augmentation de la chaleur solaire. Il ne serait pas exact de dire

there is, it is true, the same amount of heat in the case supposed; but the air is denser and more moisture-laden; the nights are warmer, because the skies are less clear, and the heat escaping from the earth is intercepted by clouds or by the transparent aqueous vapour in the air; and lastly, there is not so great a contrast between the warmth of the air and the direct heat of the solar rays. If the atmosphere of Mercury, therefore, be excessively rare, as some have supposed, so as to afford an Alpine or Himalayan climate in comparison with the tremendous heat we should otherwise ascribe to the climate of the planet, there would by no means result a state of things resembling that with which we are familiar on earth. We must not, in our anxiety to people Mercury [1] with creatures such as we know of, blind ourselves to the difficulties which have to be encountered. We cannot thin the Mercurial air without adding to the direct effects of the sun upon

que le climat d'un point situé sur les sommets des Andes et de l'Himalaya correspondît tout-à-fait à celui d'une région inférieure qui aurait la même température, car les circonstances sont très-différentes,' and so forth, sentence after sentence, paragraph after paragraph, agreeing so closely with what we had ourselves written seven years earlier that we should almost have imagined M. Flammarion was borrowing but for the absence of any form of acknowledgment.

[1] It is a strange coincidence that even this somewhat quaint notion occurred here to Flammarion in penning those remarks of his which run so closely parallel with our own. 'Dans notre anxiété de peupler ce monde d'êtres semblables à ceux que nous connaissons, nous ne devons pas pour cela nous aveugler sur les difficultés intrinsèques.' So too the line from Dante's *Inferno* occurred to him in due course. However, he omits Whewell's 'microscopic creatures with silicious coverings.'

the Mercurial inhabitants. Whether in this way we increase the habitability of the planet may be doubted, when we consider that the direct action of the sun's rays upon the tropical regions of Mercury, thus deprived of atmospheric protection, would produce a heat four or five times greater than that of boiling water. It will hardly be thought that an intense cold in the shade, or during the Mercurial night, would compensate for so terrible a heat. In fact, this view of the Mercurial climate would lead us to find a close resemblance between the inhabitants of the planet and the unfortunates described by Dante as doomed "a soffrir tormenti e caldi e gieli." It would seem hard to believe in the existence of any organised forms under such conditions, unless perhaps such "microscopic creatures with silicious coverings" as Whewell proposed to people Venus with.'

But, on the other hand, nothing is gained (as it appears to us) by assuming that Mercury may have a very dense atmosphere, heavily laden (as indeed it could not fail to be) with moisture and with cloud masses of various kinds, from the *nimbus* or rain cloud to the lightest order of clouds, the feathery *cirrus*.

A dense atmosphere ordinarily causes increase in the total amount of heat, and a diminution in the range of change, both diurnal and annual. The latter effect —that is, the tendency to general uniformity of temperature—is, in most cases with which we are familiar on earth, a favourable one. But it would hardly be favourable if the temperature thus uniformly maintained were a very high one. Suppose, for instance, our atmosphere were twice as dense as it is, and (as it would be in that case) very heavily laden with moisture, then a temperature would prevail, in the temperate and torrid zones, such as no race of men on earth could endure; but if to these conditions we superadd a sun seven times as powerful as ours, we should have a state of things which would be very mildly described by the word 'purgatorial.' The temperature of the radiating room in a Turkish hammams would be coolness to the temperature which would prevail during the midday hours in mid latitudes; and this heat could not be dry heat, like that of a Turkish bath, but would be accompanied by the action of masses of intensely hot vapour. It is not only certain that no human race could endure such conditions *as* a race, but no human being (as men are constituted now) could survive under such conditions for five minutes. We believe, indeed, that the temperature which would of necessity prevail under such a sun, with an atmosphere of the assumed density, would be such that exposure to it for a single instant would destroy any human being as certainly as immersion in boiling water.

But it seems possible—barely possible, however—that a planet might have an atmosphere so constituted as to remain almost constantly loaded with heavy masses of cloud, and in such a way that the clouds would serve as a protection from the sun's intense heat. As we have elsewhere remarked, the only climatic effect which can be associated 'with the frequent presence of large quantities of aqueous vapour in the air—or, therefore, with an ordinarily clouded state of the sky—is that of a general increase of heat. But, just as we know that a cloudy day is not necessarily, nor even commonly, a warm day, it may well be that an atmosphere so dense as to be at all times cloud-laden serves as a protection from the sun's intense heat; so that, instead of assigning dense atmospheres exclusively to the more distant planets, as some astronomers have done, we might be led to see in an envelope of great density the means of defending the inhabitants of Mercury and Venus from the otherwise unendurable rays of their near neighbour the sun.'[1]

But these efforts to show how Mercury might possibly be habitable by such creatures as ourselves are based on the tacit assumption that he must be a world inhabited by such creatures, an assumption which few astronomers in the present day would consider valid, while none would consider that any reasoning respecting the actual condition of the planet could safely be based on such an assumption, even though the assumption were reasonable and probable in itself. Let us consider what evidence we have as to the existence of an atmosphere around Mercury.

We may, not unfairly, consider first the *à priori* probabilities.

[1] The passage here cited has, like the former and several pages of the chapter from which it is taken, the advantage of corresponding satisfactorily with the views adopted by M. Flammarion. In this special instance, however, he has quoted from the present writer, with due acknowledgment, except that reference is made to the wrong work. There is something perplexing in this.

Mercury, as one of the primary members of the solar system, may be regarded as resembling the sun himself in general constitution, even as our own earth does. The planet was probably formed somewhat later than our own earth, being nearer to the sun; though it should be noted that, according to the views which are beginning to prevail respecting the development of the solar system, there is no absolute necessity for regarding proximity to the sun as an evidence of relative youth. As a much smaller planet than the earth, Mercury has probably passed far more quickly through the various stages of planetary life;[1] and if the two planets

[1] It is not commonly known (we were not ourselves aware of it until long after we had been independently led to the same conclusion) that Newton was among the first, if not the very first, to show that the larger a planet is the longer will be the various stages of its existence as a planet. 'For a globe of iron,' he says, 'of an inch in diameter, exposed red hot to the open air, will scarcely lose all its heat in an hour's time; but a greater globe would retain its heat longer in the proportion of its diameter, because the surface (in proportion to which it is cooled by the contact of the ambient air) is in that proportion less in respect of the quantity of the included hot matter.' (His meaning here may best be illustrated by example. If the larger globe has a diameter twice as great as the smaller, it has a surface four times as great, a volume eight times as great; so that it has eight times as much heat to part with; but, instead of having a surface eight times as great, as it should have in order to part with its eightfold supply of heat in the same time, it has a surface only four times as great, or half as great only as it should be for that to happen. In Newton's words, the surface of the larger is in this proportion—or the proportion of the diameter—less in respect of the quantity of the included hot matter.) 'And, therefore,' Newton proceeds, 'a globe of red-hot iron equal to our earth—that is, about 40,000,000 feet in diameter—would scarcely cool in an equal number of days, or in above 50,000 years. But I suspect that the duration of heat may, on account of some latent causes, increase in a yet less

Terra and Mercury had begun life in the same era and nearly at the same epoch, we might safely assume that Mercury had reached a much later stage of planetary development. But as it seems quite possible that Mercury may have begun planetary life far later—even several hundreds of millions of years later—than our earth, it is possible that Mercury may be very little more advanced in development than the earth, or may be in the same stage, or may even be a far younger planet in condition as well as in years. Hence we are in a position of uncertainty on the question of relative age, and must look for direct evidence on this special point and on all questions which depend upon it.

The quantity of matter in the globe of Mercury is equal, so far as can at present be determined, to about seven-hundredths of the earth's mass. Assuming his diameter to be about 3,300 miles, his volume would be about 63-thousandths of the earth's, so that his density would be greater than hers by about one-tenth. But some measurements of his globe would make his volume larger and his density about the same as the earth's. His surface is about one-sixth of the earth's. Now, his mass is about one-fourteenth of hers, so that, supposing that the mass of his atmosphere bore the

proportion than that of the diameter; and I should be glad that the true proportion was investigated by experiments.' Such experiments have shown that the period required for the cooling of a globe of red-hot iron as large as the earth would be several thousand times longer than that deduced in the above rough manner by Newton. But the principle of his reasoning is sound enough.

same proportion to the mass of the earth's, one-fourteenth the quantity of air surrounding our globe could be spread round the globe of Mercury, the surface of which is much greater than a fourteenth, being nearly a sixth, of the surface of the earth. There would be less air, then, over each square mile of Mercury's surface in the proportion of six to fourteen, or three to seven. But this air, three-sevenths only in amount according to our assumption, is drawn downwards towards the surface of Mercury by a much smaller gravitating force than is exerted upon our own air. Gravity in Mercury is about nine-twentieths only of terrestrial gravity. Taking this circumstance into account, we find that, on the assumption we have made, the atmospheric pressure at the sea-level of Mercury would be less than one-fifth (nine-twentieths of three-sevenths) of the atmospheric pressure at our sea-level. The mercurial barometer on the planet Mercury would stand only at about six inches, corresponding to the atmospheric pressure at a height of more than eight miles above the sea-level, or far higher than the height reached by Coxwell and Glaisher, when Glaisher fainted and Coxwell had barely strength left to draw with his teeth (his hands being powerless) the valve string of his balloon.

The assumption here made may be very remote from the truth. Still it seems the most probable that can be made. In one sense, of course, it is utterly improbable. No one can imagine that the quantity of atmosphere on any planet bears to that on our own earth precisely the same proportion that the mass of that planet bears to the earth's. Yet the supposition is still the most probable that we can make. In guessing beforehand where a bullet aimed at a target will fall, the most probable definite assumption we can make (whatever the skill of the rifleman may be) is that the central point of the bull's eye will be struck—assuming always that we know of no cause tending to cause the bullet to fall on one side rather than on another of that point. The chance that that precise point will be struck, may be, and generally is, exceedingly small; but it is not quite so small in any case (despite the joke commonly made that the safest place in front of a bad marksman is the point he aims at) as the chance that any other *definite point* will be struck. And precisely as the region where, in the long run, the greatest number of bullets would fall would be the region surrounding the central point, so in the case of assumptions such as we have made above, though it is utterly unlikely that any given assumption would be precisely fulfilled, yet it is more probable that the truth lies somewhere near the mean assumption than that it is far removed from that assumption. Nevertheless there is no question (unfortunately for our reliance on such reasoning) but it *may* be very far removed.

Still it seems exceedingly probable that the atmosphere of Mercury, though it may not be so rare as our assumption would make it, is at least far rarer than the earth's atmosphere.

The telescopic evidence we have on this point is satisfactory as to the existence of a Mercurial atmosphere of appreciable density, but is not sufficient to supply an answer to the question whether the atmosphere is rarer or denser than our earth's.

In the first place it seems satisfactorily shown that there is a twilight circle on Mercury. For the light on Mercury's disc, when this is seen as a crescent, half, or gibbous moon, fades off in such a way towards the boundary between the dark and illumined portions as to correspond better with the effects of atmospheric diffusion than with those due merely to the direction in which sunlight falls on different parts of the globe. And not only so, but the light spreads farther than it would if there were no atmosphere, or only a very rare atmosphere. We have not yet heard of any observation showing that the fine sickle of light, as Mercury approaches the sun's place in the sky (or what is technically called inferior conjunction), extends gradually farther and farther round until it forms a complete circle, as has been noted by Professor Lyman in the case of Venus. Until an observation of this kind has been made, it can scarcely be said that the existence of a tolerably dense atmosphere round Mercury has been absolutely demonstrated by this method of telescopic observation. Still there are few astronomers who entertain any doubts that the aspect of Mercury, especially in his crescent form, indicates the presence of an atmosphere of considerable extent and density.

Next it has been noticed that when Mercury crosses the face of the sun, or is *in transit*, the black disc of the planet occasionally appears to be surrounded by an arc or fringe, rather darker, according to some observers, but, according to others, somewhat brighter, than the sun's disc, on which, as on a bright background, the planet is projected.

Although accounts vary greatly as regards this fringe around Mercury in transit, most observers failing utterly to see it, while some see it dark and others bright, we must not too hastily reject the phenomenon as a mere optical illusion. The fact that so skilful an observer of the sun as Dr. Huggins not only noticed the ring as an arc of somewhat brighter light than that of the sun's disc, but noted it as conspicuous even when he used the strongest darkening-glasses, seems unmistakably to prove that it is a real phenomenon. We can very well understand that at different times the atmosphere of Mercury may produce different effects. At one time its absorptive action may be more than compensated by the circumstance that its refractive action brings into view light from brighter parts of the sun than lie immediately behind the planet. In that case the ring would appear rather brighter than the solar background. At other times the absorptive action would not be compensated in this way, and then the ring would appear darker than the solar background. At other times the compensation would be so nearly exact that the ring would be appreciably of the same brightness as the solar background, and so would not be recognisable. At all times the phenomenon would be difficult to recognise, so that we can quite well understand why many telescopists fail to perceive the ring at the very time when some more clear-sighted observer has noted it. Thus when Huggins saw the ring around Mercury on November 5, 1868, no other observer perceived the appendage; in fact, whereas Huggins, when not looking for it, noticed the ring, some failed to perceive it who searched specially for it with the expectation, and in some sense with the hope, that it might be discernible. Thus M. Flammarion (on all of whose observations, however, we feel disposed to look with some degree of doubt) remarks of Huggins's observation, 'Combien la vision humaine est singulière! Pendant que M. Huggins observait en Angleterre ce passage de Mercure devant le soleil, je l'observais à Paris, comme je l'ai déjà dit, avec toute l'attention possible également, et je n'ai pu apercevoir ni point lumineux ni trace d'atmosphère. Et cependant je les cherchais avec une idée préconçue. Cela ne veut point dire que l'astronome anglais et tous ses prédécesseurs se soient trompés; mais ces différences nous apprennent à ne pas nous fier à la vue dans certains cas spéciaux, comme dans ceux où le contraste joue un grand rôle. Non-seulement la vue, la sensation de la rétine, le jugement, diffèrent d'un observateur à l'autre, mais l'instrument employé entre lui-même pour une large part dans les résultats de l'observation. Le passage de Mercure du 5 novembre 1868 a été observé par plus de cinquante astronomes, en France, en Angleterre, en Allemagne, en Russie, en Italie, en Espagne, et M. Huggins est le seul qui ait vu l'auréole et le point lumineux.'

The most satisfactory evidence we have, however, respecting the existence of an atmosphere around Mercury, is that derived from spectroscopic analysis. The evidence is of the same nature, and would seem to be almost as satisfactory in character, as that afforded in the case of Mars. The light we get from a planet is of course reflected sunlight, and therefore the spectrum of a planet shows the rainbow-tinted streak crossed by dark lines which we have as the spectrum of the sun. This is the spectrum belonging to a glowing solid liquid or much compressed vaporous mass shining through vapours which, though absolutely in a state of intense heat, are yet relatively cool; though of course no one supposes Mercury himself to be a body of this sort, or to shine through an atmospheric envelope so constituted. But the sunlight which comes to us from Mercury, though in the main it can tell us only about the sun, has yet something to tell us of what has happened to its rays in their progress *twice* through the atmosphere of Mercury. When the sun's rays pass through the denser and more moisture-laden parts of our own atmosphere, they tell us, when forming a spectrum, of their passage through large quantities of the vapour of water; for they show certain dusky bands which are either wanting altogether, or much less conspicuous, in the spectrum of a high sun. Now, in the spectrum of Mercury these bands are sometimes (not always) seen, and this too when Mercury is so high above the horizon that the bands cannot be caused by moisture in our own air. We learn, then, two things—not only has Mercury an atmosphere, but there is water also on his globe in quantities large enough to load that atmosphere heavily with aqueous vapour.

We must not, however, fall into the mistake of supposing that of necessity the atmosphere of Mercury, even at the times when these bands are seen, is more heavily laden with moisture than our own air. It has been too hastily concluded that, because these bands are seen in the spectrum formed by the light of Mercury as a whole, whereas they are only seen in the solar spectrum when the sun's rays pass through the deepest parts of our own air, Mercury's entire atmosphere exerts as great an absorptive action in this way as our own air at a maximum. It must be remembered that the rays from Mercury have passed not once but twice through the atmosphere of Mercury, and that the light from a considerable portion of his illuminated disc has even passed twice through the densest part of his atmosphere. Then, again, the same rays have passed also through our own air, and though on these occasions Mercury has not been so low down that the rays have passed through the deepest parts of our air, yet, as he is never seen *shining* high above the horizon, and can indeed only be studied with advantage, so far as his light is concerned, when but little raised above the horizon, it is manifest that the absorption exerted by his own atmosphere, during the double passage of the solar rays through it, must be appreciably reinforced by the absorptive action of our own atmosphere. Add to these considerations the well-known circumstance that variations of tint are always more clearly to be recognised when all the tints under examination have been proportionately reduced in brightness, than they could be before such reduction, so that, for instance, the atmospheric bands can be more readily discovered in the spectrum of the horizontal moon (though she has no atmosphere) than in that of the horizontal sun, and we perceive that the visibility of these bands in the spectrum of Mercury affords no sufficient proof that the planet's atmosphere is more heavily laden with the vapour of water than our own atmosphere.

That the air of Mercury *is* thus heavily laden with moisture seems, however, in itself likely enough. The intense heat poured by the sun upon Mercury must cause enormous evaporation. In fact, it is difficult to imagine how any water existing on the planet's surface can escape evaporation at Mercurial midday. We may apply to Mercury now, almost unchanged, the reasoning applied by Newton (*Principia*, bk. iii. prop. viii.) —'Our water, if it were removed to the orb of Mercury, would quickly fly away in vapour; for the light of the sun, to which his heat is proportional, is seven times denser in the orb of Mercury than with us, and by the thermometer I have found that a sevenfold heat of our summer sun will make water boil'—though we cannot adopt the conclusion which Newton so confidently accepts, viz. that the materials, solid and liquid, of which Mercury is composed, must therefore be very different from the substances with which we are familiar. 'Nor are we to doubt,' said Newton, 'that the matter of Mercury is adapted to its heat, and is therefore more dense than the matter of our earth, since in a denser matter the operations of nature require a stronger heat.' We now know that we are very greatly to doubt this seemingly safe inference, or rather that it is most certainly unsound.

Looking around for any further evidence respecting the atmosphere of Mercury, and the moisture certainly present in it at times, and probably always to a greater or less degree, we find ourselves led to consider one point to which (so far as we know) we were the first to direct attention. If Mercury were generally enwrapped in great masses of cloud like the cumulus clouds of our summer skies, or indeed in clouds of any known form, it is certain that his lustre under the solar rays would be considerably greater than if he were a body like our own earth or Mars, only partially enveloped in clouds, or, like the moon, entirely cloudless. We know that clouds reflect much more of the light which falls upon them than a rock surface, even than the whitest sand-stones, and very much more light than is reflected (we speak throughout of scattered reflection, of course) from grey and brown rock surfaces. Clouds, in fact, reflect more light, or, to speak more correctly, are whiter, than any known natural substance except driven snow; and according to some estimates, the whiteness of a cloud surface is equal, under the same circumstances of illumination, to that of lately fallen snow. Probably cloud reflects diffusedly about three-fourths of the light which falls upon it, snow reflecting nearly four-fifths. But white sandstone does not reflect quite one-fourth, clay marl reflects but three-twentieths, quartz porphyry about a tenth, and dark grey syenite only about a thirteenth of the light which falls upon it. It would, therefore, be no very difficult task to determine, from the amount of light reflected by Mercury to us, whether the planet has a surface almost entirely cloud-covered, or, on the other hand, has a surface rather resembling the moon's. For if the planet were entirely cloud-covered it would reflect three-fourths of the light which falls on it, whereas if the surface were constituted exactly like the moon's it would reflect only about a sixth of the light falling on it. In the former case Mercury would shine with four and a half times as much lustre as in the latter case ; and so great a difference as this would be readily recognisable even in so delicate an observation as the measurement of a planet's total lustre, or rather its comparison with the lustre of other sources of light.

Now, it so chances that the light of Jupiter has been very carefully measured, and appears to be nearly equal to that which this planet would reflect if it were entirely enwrapped in dense masses of cloud. Thus, if we can compare Jupiter and Mercury when they happen to be near each other upon the sky, then, taking fully into account the size of each planet, their distances from the sun and from our earth, and so forth, we can determine whether Mercury's lustre corresponds more nearly to what he would have if cloud-covered, or to what he would have if his visible surface were like our terrestrial rocks. I was able, on February 23, 1868, to make a comparison of this kind. The two planets Mercury and Jupiter were very close together (on the sky, of course, not in reality), Mercury being nearly at his brightest, whereas Jupiter, then nearly in conjunction with the sun, was considerably less bright than when in opposition—that is, shining highest above the horizon at midnight. Now, it could be readily calculated that under these conditions the lustre of Mercury should have surpassed that of Jupiter fully as 3 to 2; but in reality Jupiter shone far more brightly at the time than Mercury.[1] The inference is obvious. Mercury's surface cannot be to a great extent covered by clouds, but must for the most part be either land or water. We must, then, dismiss the idea that the intense heat of the Mercurial sun is mitigated by the interposition of unbroken envelopes of clouds.

[1] M. Flammarion made a similar observation six days earlier—viz. on the evening of February 17, 1868. 'On that day,' he says, in the recently published work from which I have already quoted, 'the two planets were close together on the sky (in perspective), and though Jupiter was far removed from his epoch of greatest brightness, yet Mercury, which was exactly at his brightest, was far less brilliant than Jupiter. At the same time Venus was also close by those two planets. She eclipsed both' (in the poetical, not the astronomical sense, of course) 'by her intense white lustre; beside Jupiter she produced the same effect as an electric light beside an ordinary gas flame (*bec de gaz*). She shone with light as white and limpid as that of a lustrous diamond; Jupiter's was yellowish and almost red; Mercury's less brilliant still than Jupiter's, and more ruddy.' These peculiarities of colour are very significant.

When we consider other relations presented by Mercury—the length of his year, the nature of his rotation, both as affecting diurnal and annual phenomena, and so forth—we find little to encourage the idea that he can at present be the abode of any forms of life such as we are familiar with on earth.

We have already considered the effect of the shortness of the Mercurial year in intensifying, by making more rapid, the changes in the supply of solar light and heat; but its effect in connection with seasonal changes must be still more marked. It appears, from observations made by Schröter, that Mercury turns upon an axis inclined fully 70 degrees from uprightness to the plane in which Mercury travels. The corresponding inclination in our earth's case amounts, as everyone knows, to about 23½ degrees only; and to this inclination our seasons are due. If the inclination were greater, the variation of the seasons would be greater. The sun's midday elevation, at any place on the earth, ranges during the year from 23½ degrees above to 23½ degrees below its mean value, or through 47 degrees in all, corresponding to rather more than half the range from horizon to the point overhead. By this considerable amount does the midday elevation of the summer sun exceed that of the winter sun. In Mercury the corresponding range would be twice 70 degrees, or 140 degrees, if there were room for such a range. But of course in most latitudes there cannot be, for this range corresponds to more than three-fourths of the distance from the southern horizon across the point overhead to the northern horizon.

There is, however, another, and perhaps a simpler, way of viewing the matter. In the summer of our hemisphere the earth presents towards the sun a face on which the north pole is well brought into his view, the whole of the arctic regions, extending for a distance of 23½ degrees from the pole all round it, being at midsummer fully in the sun's view, and the antarctic regions of similar extent hidden from the sun's rays. At midwinter of our hemisphere the antarctic regions are in view, and the arctic out of view, of the solar orb. Now, in Mercury's case the same state of things prevails, but at intervals of 44 days instead of 182½, while the arctic and antarctic regions, instead of extending only 23½ degrees from the poles, extend 70 degrees from them, or to within 20 degrees of the planet's equator. Over these two enormous portions of the planet's surface the characteristic peculiarity of arctic regions presents itself—viz. there is no night at midsummer and no day at midwinter. But the day in these arctic regions is something very different from the day in our arctic regions. Not only does the enormous sun of Mercurial skies blaze above the horizon for the whole day, but it attains at midday in the polar regions, and for a distance of 40 degrees all round the pole, a height of more than 70 degrees (8 degrees higher than our summer sun at midday), while at nominal midnight in these portions of the arctic regions the sun is never lower than 30 degrees. As for the remainder of the arctic regions, we shall presently consider their fortunes more particularly.

Turn next to the regions around the equator. To a distance of 70 degrees on either side of the equator, or to within 20 degrees of the poles, we have the characteristic peculiarity of the torrid zone—viz. the sun twice in the year vertically overhead at noon. The midday sun of Mercury must be a tremendous phenomenon, and to support its fiery blaze living creatures should be well adapted to endure intense heat. A very few minutes of its action would kill the strongest living man. But there is a mid interval between the recurrences of these midday terrors, during which the midday sun is very low, and a quite different state of things must prevail. At the equator in winter and summer (nominal, of course, for at the equator of a planet spring and autumn are always the periods of intensest heat) the midday sun has an elevation of only about 20 degrees; and it should seem that any atmospheric arrangement by which the intense heat of a vertical sun would be rendered endurable in Mercury would make the days of summer and winter at the equator intensely cold. Not, indeed, that they can be imagined cold to creatures such as we are, but to creatures capable of enduring unscathed the blaze of a vertical Mercurial sun they would appear so. Now, the change from intensest heat to the next cold season is completed in three weeks only of our time. Certainly very strong constitutions must be required to support changes so vast and so rapid.

But it is only at the equator itself of Mercury, or close to it, that no greater changes than these have to be endured, if indeed there are living creatures on Mercury. We have seen that in the polar regions there is an intense summer heat, lasting throughout the entire day, while at midwinter, only six weeks later, the sun does not rise above the horizon, or even approach it, during the whole day—or rather during the twenty-four hours. This involves, of course, a change much more terrible than that which occurs along the equatorial zone, though not quite so rapid; and it may safely be said that no creatures in the remotest degree resembling any we know of could bear such rapid alternations of intense heat and cold. But the zones, fully fifty degrees wide, where the torrid zone overlaps the arctic regions, are certainly not more desirable abodes, according to our ideas; for here the characteristic properties of both arctic and torrid regions are combined. Twice in each year there is a vertical midday sun; at one season in each year there is no day, and at another season there is no night, throughout the whole of the twenty-four hours. And all these vicissitudes occur in a year lasting three days less than one of our seasons!

Unless we adopt the fanciful notion once thrown out, that the inhabitants of Mercury are exceedingly mercurial in their habits, passing from one zone to another as the sun's elevation changes, so as always to occupy regions where there is neither excessive heat nor excessive cold, we can hardly imagine that this planet can at present be the abode of life. Nor, indeed, does it appear altogether probable that life would be pleasant on Mercury even after a few millions of years have passed and the sun's globe has cooled down to about one-seventh of its present temperature (in which state he would supply Mercury with as much heat as we at present receive). The peculiarities of the seasons described above would still remain, for we know of no force competent to greatly change the position of the polar axis of a planet, whether by a displacement of the entire planet through some external shock, or by the action of internal forces displacing the crust of the planet.

If indeed there is any planet in which volcanic changes (or, to speak more correctly, Vulcanian forces) might effect a considerable change in the position of the polar axis, it is Mercury; for, according to observations made by Schröter, this planet would appear to have mountain ranges or high table-lands rising above his mean level (his sea-level, if he have any) fully fourteen miles, which would correspond to a height of more than thirty miles on our earth. Compared with such mountains our Himalayas and Andes are only hills. But even mountains so enormous, and Vulcanian forces competent to upheave large tracts now below the Mercurial seas to as great a height, or even greater, would account for only slight displacements of the polar axis of Mercury; and a very great displacement would be required to make the Mercurial seasons resemble in range and character those of our own earth.

It may be mentioned, in passing, that one phenomenon of Mercury, if real, might fairly be regarded as indicating Vulcanian energies compared with which those of our own earth, or even those which we have imagined in the preceding paragraph, would be as the puny forces of a child compared with the energy of a giant. It has been supposed that a certain bright spot seen on the black disc of Mercury when the planet is in transit, indicates some sort of illumination either of the surface of the planet or in its atmosphere. In its atmosphere it can scarcely be; nor could any auroral streamers on Mercury be supposed to possess the necessary intensity of lustre. If the surface of Mercury were glowing with the light thus supposed to have been seen, then it can readily be shown that over hundreds of thousands of square miles that surface must glow with an intensity of lustre compared with which the brightness of the lime-light would be as darkness. In fact, the lime-light is absolute blackness compared with the intrinsic lustre of the sun's surface; and the bright spot supposed to belong to Mercury has been seen when the strongest darkening-glasses (or other arrangements for reducing the sun's light) have been employed. But there can be no manner of doubt that the bright spot is an optical phenomenon only. Regarded as a Mercurial illumination, it is unquestionably as utter a myth[1] as the celebrated satellite of Venus, by which

[1] One ingenious but unscientific theoriser has suggested that the bright spot on Mercury may be the image of our earth mirrored on the surface of a metallic and possibly glass-enveloped planet!

astronomers were so often perplexed during last century, or, in other words, during the era of the first and necessarily imperfect telescopes of considerable size and power.

No choice seems left but to adopt one or other of two general inferences respecting the possibility of life upon the fiery Mercury. Either we must believe that the conditions under which life can exist vary much more widely than anything known here (as respects either the present era of the earth's history or those remote ages in the past when her condition was probably very different) would suggest, or else we must admit the probability that this small planet is not only at present unfit to be the abode of life, but cannot have been inhabited in any past era, and can never become habitable hereafter.

To many the first of these inferences will commend itself as the more satisfactory. It appears to some altogether inconceivable either that a planet can have been made for any other purpose but to become the abode of living creatures, or that, regarding a planet as only fitted for such a purpose—as having no other conceivable use, if we may so speak—any planet can fail of fufilling that purpose. Such persons are barely willing to accept the opinion which we have advocated as in

The only objection to this view (at least the only one we need notice) is that the greatest possible amount of light we could receive from such an image, assuming Mercury to be exceedingly well polished and of the best mirror-metal, would correspond to that of a star which would just be rendered visible in the darkest and clearest night with a telescope 25 *feet* in aperture.

reality according best with known facts, that each planet has its special period of fitness for life, which period is short in duration compared both with the preceding period of preparation and the sequent periods of decadence and ultimately of deathlike unchangeableness. But they are utterly unwilling to accept the possibility which suggests itself to all who consider the full evidence in these matters—that a considerable proportion of the orbs which people space are not only not inhabited now, but never have been inhabited and never will be.

Yet to those who consider the subject apart from conceptions based on our own insignificance both as to space and as to time, a planet in such respects differs in degree only, not in kind, from an embryo or a seed. Granting that the support of life is the special purpose for which alone a planet is suited, that cannot be more certainly known than that the special purpose for which a seed is formed (in every detail of its structure) is that it should eventually grow into a plant of its own kind. Even adding to this special purpose of a seed's structure such other purposes as seeds fulfil in becoming parts of the food of men and animals, or in being employed to make various substances of use to man, is it not the case that multitudes of seeds fulfil none of these purposes? We know that of seeds even which are preserved for sowing some fall by the wayside and are devoured by the fowls of the air, and so become of use after a fashion. But 'some fall in stony places, where they have not much earth, and forthwith they spring up, because they have no deepness of earth, and when the sun is up they are scorched, and because they have no root they wither away.' And again, 'some fall among thorns, and the thorns spring up and choke them.' Not all fall into good ground and bring forth fruit, whether a hundredfold, or sixtyfold, or thirtyfold. May not the same be true also, though on so much larger a scale, of planets? In His eyes to whom one day is as a thousand years, and a thousand years are as one day, we may be sure that the mightiest orb in space is as a grain of seed, a grain of seed as the mightiest orb. Of the planets which were made (let us assume to support life), even as the seed after its kind, some may spring too soon into being, when the fires of the youthful sun are poured too scorchingly upon them for life to come into existence. Others (like the zone of asteroids) may be scattered in such sort that they never even spring into full being as planets. Only a proportion may, like our own earth, come into being in pleasant places, where neither too intense a heat nor unendurable cold may afflict creatures living upon them; and thus they may bring forth life abundantly, after their kind, and in such degree as may be suited to their position in the planetary universe. Reasoning which would cause us to reject such conclusions as inconsistent with our conceptions of the fitness of things would equally cause us to reject as incredible the waste of multitudes of seeds, were it not that we know how many seeds are choked by thorns, how many, after sprouting into life, are scorched and withered by the sun. Astronomy gives many valid, if not demonstrative, reasons for believing that what thus happens within our ken on a small scale, happens also, on a wider scale, among the orbs which people space.

THE PLANET OF WAR.

AMIDST wars and rumours of war, the planet which has for its symbol the spear and shield of the old Assyrian warmen approached in October 1877 one of those points of its epicyclic orbit about the earth where it is at its nearest to us. In the earlier part of the Crimean War, Mars shone in our midnight skies, though not so splendidly as he will shine in August and September of the present year (1877). In the early spring of 1854,

> At a time of the year
> When the face of night is fair on the dewy downs,
> And the shining Daffodil dies and the Charioteer
> And starry Gemini hang like glorious crowns
> Over Orion's grave low down in the west,

the spirit of Maud

> . . . Seem'd to divide in a dream from a band of the blest,
> And spoke of a hope for the world in the coming wars—
> . . . and pointed to Mars
> As he glow'd like a ruddy shield on the Lion's breast.

The poet expressed the feeling of the day, though the hope of which he sang was not the hope with which men now watch the signs of war. But if Mars were in truth the planet of war, if his influence poured from near at hand upon the nations of this earth, excited them to war and bloodshed, it might well be feared that the closing months of 1877 would bring desolation on many fair terrestrial fields. For Mars had not blazed so fiercely in our skies since 1845, nor will he so shine again for forty-seven years, as during the last days of August and the opening days of September. Moreover, twice during his time of greatest splendour his rays were closely conjoined with those of the malignant planet Saturn, the greater Infortune, as Mars himself is the lesser Infortune, of astrological systems.

The ruddy hue of this planet, justifying the evil qualities attributed to it by nations believing in planetary influences, has been noted from the earliest times. The Greeks called Mars the fiery planet; the Hebrews gave to it a name signifying 'enkindled;' the Indians called it Angaraka, or burning charcoal, and sometimes Lohitanga, or the red orb. Ruddy stars also were compared with Mars, as the chief of all the ruddy stars,—so that the name Antares given to the star which glows like a fiery coal in the heart of the Scorpion, signifies that in ruddiness that star is a rival of Mars or Ares.

Recent researches among the ruins of Nineveh have brought to light cuneiform inscriptions relating to the celestial bodies, and among others to the planet Mars. It would appear that a treatise, in sixty books, called *The Observations of Bel*, belonged formerly to the public library of Nineveh. Its date cannot have been later than the seventeenth century before our era, and the observations recorded in it extend over more than 500 years, so that the earliest bore date about 2540 B.C. One of the books was devoted to the pole star—not our present pole star, but the star Alpha of the Dragon, at that time the bright star which lay nearest the pole of the heavens. Another book was devoted to Venus, a third to Mars. We find that even at the remotest time to which these records relate, that is, more than 2,500 years before our era, the planet Mars presided (as a deity) over the third day of the week, the other planets ruling the days in the order indicated by the present nomenclature, the Sun presiding over Sunday, the Moon over Monday, Mars over Tuesday (or Mardi), Mercury over Wednesday (or Mercredi), Jupiter over Thursday (or Jeudi, Jove's day), Venus over Friday (or Vendredi), and Saturn, the gloomiest and most malignant but also the most powerful of the planetary deities, over Saturday, the sabbath day, when, owing to his evil influence, no work could safely be undertaken. Doubtless Tuesday was as rigidly set aside for the initiation of all warlike enterprises as Saturday for the avoidance of all labour whatsoever.

If only astrology had been a true method of prediction, the discovery of the true nature of the solar system would have brought within our range much fuller information respecting the other planets, and in particular the planet Mars, than we are ever likely to possess. Astrologers claimed such perfection for the principles of their art, that the whole history of our earth might have been predicted from the planetary configurations alone; and indeed they were very successful in showing that all past events corresponded with the aspect of the heavens when they occurred. Now, if other planets thus influence the fortunes of our earth, which is itself one of the planets, it follows that each of the planets is in like manner influenced by the positions and motions of the rest. But these can be quite easily calculated. Therefore the fortunes of the inhabitants of every planet can be determined, and the entire past history of each planet can be read by terrestrial astronomers. Only one circumstance must be ascertained telescopically. (At least so it appears to us, for we confess we are not such adepts in the methods of astrological divination as to be quite sure whether astrological principles, properly applied, might not have determined everything which the telescope has revealed. As a mere matter of fact, astrology discovered nothing of this kind. But that is the merest detail.) It should be known how a planet is posed in space, what are the pole stars of its northern and southern hemispheres, and at what rate exactly it rotates upon its axis. For the astrologer, in determining the future fortunes of his 'native,' or in calculating the native's past history, has to take into account the aspect of the star-sphere at the moment of the native's birth, as well as at the critical stages of his career; and to do this properly account must be taken of course of the hour and of the position of the pole of the heavens.

We do, however, know fairly well the position of the axis of Mars, and we know the length of his day within the tenth part of a second, so that, if only astrology were a sound method of divination, we might learn much of the past history and of the future fortunes of this planet. As De Morgan has remarked in an article on astrology in the 'Penny Cyclopædia,' 'we have lost,' in the rejection of astrology, 'a charming opportunity of discovering what goes on in other planets.'

The astronomer who watches, during the approaching close approach of Mars, the slowly rotating lands and seas of the planet, can scarcely, however unimaginative he may be (and we fear it is an essential requisite of the surveying astronomer that he should be as free from imagination as a man well can be), avoid the thought that contests such as have raged upon our earth for the possession of various regions of our planet's surface, may be in progress out yonder in space. Armies may be desolating the fairest regions of Mars at the very time when they are under the telescopic survey of the terrestrial observer. Warlike fleets may be urging their way across those seas and straits which our astronomers have marked down in their charts of the planet. We may hope, if we choose to forget our own experience of 'Nature red in tooth and claw with ravine,' that in yonder peaceful-looking world there is peace among all creatures. But our own earth, amid the fiercest tumults and the most desolating wars, presents to the other worlds that people space the same peaceful scene. Distance lends so much, at least, of enchantment to the view. The sun himself, over every square mile of whose surface turmoil and uproar prevail compared with which the crash of the thunderbolt is as silence, and the fiercest blast of the hurricane as absolute rest, looks calm and still in our skies, and even in the telescope shows signs of activity only to the mind's eye, none that our natural vision can appreciate.

It is a strange thought, too, that expeditions such as man makes to discover the hidden places of the earth may be in progress in other planets. Some among those lands and seas of Mars, which the astronomer contemplates in the ease and quiet of his observatory, may not as yet have been seen by inhabitants of Mars, because of the dangers which prevent access to them. We may well doubt, for instance, whether the bravest and most enterprising Martialists have yet succeeded in reaching either pole of the planet. Our eyes have rested on those polar regions, even on the very poles themselves, of the planet. But so, an observer on Venus, possessing optical instruments of adequate power, could see, on turning them upon our earth, those terrestrial polar regions which the most daring of our voyagers have in vain attempted to reach. And as the eyes of creatures in other worlds may thus have looked upon regions of the earth of which we know nothing from direct observation, so the eye of man has rested on the poles of a planet which is never at a less distance than 33,000,000 miles, while the inhabitants of that planet, if such there are, may have been foiled again and again in all attempts to penetrate within their polar fastnesses.

We wonder, in passing, whether the idea has ever occurred to the inhabitants of Mars that Martian regions have been made the subject of a war, and a somewhat lively war, though of words only, among terrestrial astronomers. Such has actually been the case, insomuch that, if analogy may be our guide, astronomers in Mars and Venus are not improbably contending about the distribution of the four quarters of our earth, and our principal seas, and lakes, and islands, and peninsulas, among living and dead celebrities in those planets. The story of a recent short but sharp terrestrial war over the lands and seas of Mars is not without its lesson, even if that lesson be only a response to the time-worn question, 'Tantæne animis cælestibus iræ?' It would seem that an English student of astronomy, who had found occasion often to refer to Martian regions until then unnamed, had for convenience assigned to these regions, after charting them (a work of some labour and difficulty), the names of those astronomers whose observations had thrown light upon the geography of the planet—or its areography, as, if pedantically inclined, we may name what corresponds with the geography of our earth. Thus to Sir W. Herschel one continent was assigned, to Secchi another, to Mädler a third, and to Dawes (the 'eagle-eyed' observer to whom we owe the most exact observations of Mars yet made) a fourth. To divers other astronomers, all observers of the planet, various lands and seas were assigned. This was not done with the idea of honouring those astronomers, but simply of giving convenient names to features which have often to be referred to. A Belgian astronomer, Dr. Terby, of Louvain, who has laboriously examined and compared an immense number of pictures of Mars, adopted the nomenclature just referred to, adding one or two names (including that of the author of the English chart), but making no changes. Unfortunately, however, he had somewhat misapprehended the object of the names, and described them as 'in honour of' such a one's labours, 'in recognition of' the discoveries of such another, and so forth. This proved too much for the patience of a French writer on astronomy, who found neither continent nor ocean (as it chanced) assigned to any French observer, though large tracts of land and sea were given to Laplace, Leverrier, Arago, and other distinguished Frenchmen. He therefore incontinently reconstructed the chart, altering it in many respects (all the alterations, singularly enough, corresponding more or less closely with Dr. Terby's suggestions as to what might have to be done when Mars was re-examined). He called this chart his own, and proceeded to re-name most of the lands and seas. He treated some English observers rather contemptuously, dismissing Sir J. Herschel altogether, relegating Dawes to a small sea, De la Rue to another, Lockyer to a third (all three seas close together). The most marked feature of all, a dark sea, shaped somewhat like an hour-glass, had been assigned to Kaiser, a German astronomer, who had made many interesting observations of the planet. M. Flammarion dismisses the

German to a corner of that sea, and leaves the sea itself without any name except one descriptive of its shape—possibly intending that the name of a French writer on astronomy should fill the space.

On this Dr. Terby of Louvain rose indignant. In astronomic ire and areographic grief, he solemnly denounced the new nomenclature. To say truth, he had some reason to be annoyed, because his labours had been freely used with a form of acknowledgment which, though seemingly profuse, by no means did justice to his claims. 'Nine times,' said M. Flammarion, 'does the name of Dr. Terby appear in my account of the lands and seas of Mars.' 'I would you had mentioned it once only,' retorts Dr. Terby, 'with the statement that the account is entirely taken from my labours,' where it is not borrowed from the before mentioned English astronomers. M. Flammarion promises, in return, never to mention Dr. Terby again. '*Mea culpa*: je ne le ferai plus,' he says, adding, as a pleasant parting word, 'à tout bien prendre cependant, il n'y a rien d'étonnant à ce qu'on se bataille à propos de Mars; espérons qu'il n'y aura pas de sang versé, et que la colère du petit lion belge se calmera d'elle-même.'

Let us turn, however, from these small bickerings to the consideration of the planet itself. We have already[1] discussed two theories of the planet Mars regarded as another world. One is the theory that he is at present inhabited, and that too by creatures

[1] See *Cornhill Magazine* for May, 1871, 'Life in Mars;' and for July 1873, 'A Whewellite Essay on Mars.'

which, though they may differ very much from the inhabitants of this earth in shape and appearance, may yet be as high in the scale of living creatures. In particular this theory assumes as probable, if not certain, the belief that among the inhabitants of Mars there are creatures endowed with reason. According to the other theory, which we have called the Whewellite theory, Mars is altogether unfit to be the abode of creatures resembling those which inhabit our earth; neither vegetable nor animal forms known to us could exist on the planet; in fine, 'all the conditions of life in Mars, all that tends to the comfort and well-being of Martian creatures, must differ utterly from what is human on earth.' We have also in our essay on 'Life past and future in other Worlds' (in the 'Cornhill Magazine' for June, 1875) considered a general theory which in our opinion is far more probable than either the Brewsterian or the Whewellite—the theory, namely, that each planet has a life-bearing stage, but that the duration of this stage of its existence, though measurable perhaps by hundreds of millions of years, is yet exceedingly short by comparison with the duration of the preceding stage of preparation and the sequent stage of decay and death. From the direct application of the laws of probability to this theory, the chances are shown to be very small indeed that life exists at this present time on any planet selected at random and without reference to what observation has revealed. Precisely as, when we know that a bag contains several thousand black balls and only a few white ones, the chance that a ball taken at random is a white one is exceedingly small; so, the period of a planet's fitness for life being short compared with the preceding and following stages, the chances are very small that this present time, which is, so far as other planets are concerned, taken at random, falls within the period of any given planet's fitness to be the abode of living creatures. The telescope and the spectroscope may correct this inference, just as, on looking at a ball taken from such a bag as we have described, the drawer of the ball might find to his surprise that he had taken one of the white ones, few though they were compared with the black ones. But *apart* from such observations, the chances must be regarded as exceedingly small (according to this theory) that any given planet is at this present time inhabited. Nevertheless, two conclusions, according well with ordinary conceptions as to the fitness of things, follow from this theory:—First, our earth is but one among many millions of worlds inhabited at this present time. Secondly, every planet is at some time or other, and for a very long period, the abode of life. These three points—the small probability (apart from telescopic observation) that any given planet is inhabited now; the great probability that many millions (out of thousands of millions of planets) are inhabited now; and the equally great probability that every planet has been, is, or will be inhabited—are demonstrated in the third of the essays above mentioned. That essay presents the view towards which the present writer had been gradually led—from the Brewsterian theory which he accepted until 1871, through the Whewellite, towards which he had inclined until 1873, when finally the intermediate theory seemed pressed upon him by overwhelming weight of testimony.

Our present purpose is to show more particularly how this theory accords with what is known respecting the planet Mars. We wish also to show how both the lines of reasoning which had been before employed, one pointing to the Brewsterian theory, the other to the Whewellite theory, converge, in the case of Mars, upon this intermediate theory.

In the first place, we saw, in considering the conditions which favour belief in the existence of life in the planet Mars, that he presents the clearest possible evidence of being one in origin and structure with our own earth. We cannot tell what the nature of the soil of Mars may be, but its generally ruddy tinge—so well marked that, though the telescope shows an almost equal part of the surface to be greenish in hue, the red prevails, giving to the planet as seen by the naked eye its obvious red colour—seems to show that it resembles the red sandstone of our own earth. This, we know, is one of the older geological formations, and if we could safely compare terrestrial with Martian geology, or, let us say, geology with areology, we might almost be tempted to find in the present prevalence of a tint belonging to one of the earlier of our terrestrial formations an argument in favour of the theory that Mars passed through fewer stages of development during its life-bearing condition than our earth, and that thus the later formations of our earth's surface are wanting in the surface of Mars. This reasoning would not be very safe, however; it implies a resemblance in details which is unlikely, the observed rule of nature seeming, so far as we can judge, to be similarity in generals, variety in details. We may well believe that the ruddiness of the soil of Mars is due to the same general cause as the ruddiness of our red sandstone—the general prevalence of certain organisms; but neither the actual character of this particular formation, nor its position in the terrestrial series of strata, can be safely predicated of the ruddy formation constituting the chief part of the visible land surface of Mars. Few will now suppose, with a French writer, that the ruddiness of Mars is due to the colour of vegetation there. A certain support is given to the idea by the circumstance that the degree of ruddiness is variable, and is somewhat greater during the Martian summer than in spring and autumn. In this sense we may say of the summer of Mars with the poet Wendell Holmes—

> The snows that glittered on the disc of Mars,
> Have melted, and the planet's fiery orb
> Rolls in the crimson summer of its year.

But the ruddiness of the planet's summer—which will be well marked this year, for on September 18, only eleven days after its time of nearest approach and greatest splendour, it will be Midsummer's day for the southern half of Mars—can be otherwise and better explained than by supposing that the Martian forests glow with fiery foliage during the summer days. We can see, as the summer proceeds, the white mists which had hidden the planet's lands and seas breaking up, and the features of the surface being gradually revealed with more and more distinctness. It is to the disappearance of these mists and clouds, not to the red leaves of Martian trees, that the change in the planet's colour must most probably be referred.

We have less reason for doubt as to the nature of the greenish markings. The spectroscope, as we have already explained in 'Life in Mars,' shows that the air of Mars is at times laden heavily with the vapour of water. We can no longer therefore follow Whewell in doubting the real nature of the green parts of the planet, or refuse with him to accept the explanation of the white polar markings long since advanced by Sir W. Herschel. Undoubtedly wide seas and oceans, with many straits and bays and inland seas, exist on Mars. Snow and ice gather in the winter time about his polar regions, diminishing gradually in extent as summer proceeds, but never entirely disappearing.

Thus we are not left doubtful as to the general resemblance of Mars, so far as the structure of his surface is concerned, to the earth on which we live. He has a surface of earth, probably in large part formed by deposition at the bottom of former seas and subsequently raised above the sea-level by subterranean forces, or rather caused to appear above the surface by the effects of the gradual shrinkage of the planet's crust. Of the existence of Vulcanian energy we have unmistakable evidence in the fact that lands and seas exist, for a continent implies the operation of Vulcanian forces. The shapes, too, of the outlines of the lands and seas indicate the existence of mountain ranges, and these, too, of considerable elevation. Then we have the presence of water, and of a stable atmosphere in which the vapour of water rises. It seems no daring assumption to suppose that this air is constituted much like our own air. In the first place, if the air were formed of other gases, the spectroscope would probably reveal their existence, which has not happened; and secondly, with the evidence we have of a general similarity of structure and origin, an atmosphere of nitrogen and oxygen would naturally be formed while the planet was developing to its present condition, and would remain after other constituents of the planet's primeval atmosphere had been removed. For a similar reason we may safely infer that the greenish hue of the water implies the presence of the same substances, though not perhaps similarly proportioned, which are carried in suspension in our oceans, and give to them their green, green-blue, and blue tints.

It is important to notice these general resemblances, either demonstrated or safely to be inferred. We no longer propose to deduce from them the conclusion that the planet's present condition is like that of our own earth. We might, indeed, dwell on some considerations which naturally suggest themselves here. We might see in imagination the waves of those distant seas beating upon the long shore-lines, and hear 'the scream of a maddened beach dragged down by the wave.' We can imagine the slow progress of the Martian day—the mists of morning gradually clearing away as the sun rises; the winds raised by the midday heat, zephyrs murmuring among the distant hills or blasts roaring loudly over desolate rock-bound seas; the gathering of clouds towards eventide, though probably to pass from the skies at night (because condensed by cold), leaving the same constellations we see to shine with greater splendour through a rarer atmosphere. We can imagine all this, because we know from what the telescope has revealed that such must be the changes of the Martian day. We see in the telescope the long white shore-lines, the clearing mists of morning, the gathering mists of night—and we know that there must be air currents in an atmosphere undergoing such changes. There must be rain and snow and hail, and electrical disturbances—thunder and lightning at times—besides tornadoes and hurricanes, blowing probably more fiercely than our own, though their destructive effects must be less because of the greater tenuity of the Martian air.

But while we recognise in imagination the progress of such events as these, we must not forget that for countless ages in the past mighty processes of disturbance and continuous processes of steady change took place in our earth when as yet there was no life, nor that probably life will have ceased to exist on this earth millions of years before the land and sea and air will cease to be the scene of nature's active but unconscious workings. We cannot deduce from the mere fact that if living creatures existed on Mars they would witness such and such phenomena which are familiar to the inhabitants of earth, the conclusion that such creatures do exist there. We do not assert that no such creatures exist there. Our theory of life in other worlds does not require that any given planet should be shown to be uninhabited. Nevertheless, there are so many reasons for regarding the fulness of Mars's life-bearing season as belonging to a very remote past, that it is necessary to note the insufficiency of the mere evidence of the activity of nature's unconscious forces to prove the existence of living conscious beings on the planet.

In fine, the arguments by which, in the essay on 'Life in Mars,' we endeavoured to indicate the probability of the planet's being inhabited, prove only that the planet had an origin like our earth's and is similarly constituted.

On the other hand, the arguments by which, in the essay entitled 'A Whewellite Theory of Mars,' we endeavoured to show that Mars is not in a condition fit to be the abode of life, tend to show that, while similar to the earth in origin and structure, Mars is in a far later stage of planetary development.

One of these arguments, indeed, does not relate to the condition of the planet itself, but to its position with reference to the sun. Being much further from the sun than we are, the planet receives much less direct heat. The supply is partly dependent, however, on the planet's condition; for if the air of Mars is very rare, then apart from the diminished supply there is a more rapid cooling owing to the readier radiation of heat into space. But in any case the supply cf solar heat has to be considered as one of the factors of a planet's condition, considered with reference to the question of habitability. If through its inherent heat the planet Mars was once as warm on the whole as the earth now is, that heat making up for the smaller supply of solar heat, then it seems reasonable to believe that the creatures inhabiting the planet were so far like those now existing on our earth that the same degree of heat suited their requirements. If then we find reason for believing that *now* the inherent heat of the planet is much less than that of our own earth, so that on this account the descendants of those creatures would be unable to exist unless great modifications had taken place in their requirements, which modifications seem outside any effects which could be attributed to natural selection, then the inference that therefore these races of creatures have died out is certainly strengthened, and in no small degree, by the fact that the supply of heat received from the sun is much smaller in the case of Mars than in the case of our earth. Seeing, then, that the average daily supply of light and heat on Mars (taking square mile for square mile of his surface) is less than the average daily supply on our earth in the proportion of two to five, we have here a strong argument, we will not say in favour of the belief that Mars is not now inhabited, but in favour of the belief that the duration of the life-supporting era has been, is, or will be much more rapidly shortened than in our earth's case, by the cooling of his globe. For the life-destroying influence of the cooling is much more effectively strengthened in his case than in our earth's, by the effect of distance from the central source of light and heat.

All the other circumstances in the condition of Mars point directly to the conclusion that Mars must have long since passed his planetary prime. His orbit being outside the earth's, he was probably formed far earlier, though this is not so certain as it was held to be when Laplace's nebular theory was first advanced. It is, however, very unlikely that he began to be formed later; and as he is much smaller, he would probably be fashioned more quickly. It is still more probable, in fact very much more probable, that he cooled much more quickly than the earth. His mass is not much more than a ninth of hers, while his surface is only about one-third of hers. He had, then, originally, even if of the same temperature when first formed, only one-ninth her amount of heat to distribute, so that, if he had radiated away at one-ninth of her rate, the supply would have lasted as long. Pouring it away at one-third of her rate—for the radiation taking place from the surface is proportional to the surface—he parted with it three times faster than he should have done in order to cool at the same rate as the earth. Hence he cooled three times faster than the earth, and must have attained a condition which she will not attain until three times as long an interval has elapsed from the era of her first existence, as has already elapsed. Since most geologists assign many hundreds of millions of years to the last-named period, and all agree that it must be measured by many millions of years, it follows that twice as many hundreds of millions of years must elapse if the former are right, but only twice as many millions of years if the latter are right, before our earth will be in the same condition as Mars. In reality our argument is not at all affected by the difference of opinion among geologists in this respect. For the question is of the condition of Mars, not of the number of years which may have elapsed since he was in the same condition as our earth, or of the number of years which may have to pass before our earth will be in the same condition as Mars. Whether Mars requires hundreds of millions, or millions, or only thousands of years to pass through one stage of its planetary existence, our earth requires about three times as long; and taking the entire development of Mars and the earth (assuming they began planetary existence together), Mars must be some three times as far on the way towards planetary decrepitude and death as our earth.

Only one circumstance in the discussions of geologists on the question of the time required for the development of a globe like our earth, bears very strongly on our opinion as to the existence of life on Mars. It is not altogether certain that the life-bearing era of a planet is exceedingly short compared with the era of growth and preparation, and the era of decrepitude and death. So far, indeed, as astronomical considerations are concerned, we perceive that the fashioning of a planet must be a process requiring an enormous length of time. The slow aggregation of nebulous matter, the separation of ring from ring, the breaking up of a ring into separate nebulous masses, and the gathering of each ring of them into a single mass, must have proceeded very slowly; and few who consider all the circumstances of the case will doubt that hundreds of millions of years must have elapsed between the time when first the matter of a future planet began to have separate existence, and when at length it was all gathered together in a single mass. But what followed—the gradual contraction and cooling of that mass till it became a true planet, the gradual cooling of the planet until its surface became separable into land and water, the further cooling till life became possible, the progression of life through all its various stages till earth and sea and air had each their various races of living creatures—all these stages of the planet's existence belong to the domain of geology and biology, not to that of astronomy. Doubts have arisen respecting the duration of these eras, and as yet these doubts remain. Nor have biologists as yet determined how long life may be expected to continue upon our earth. Some see already the signs of what may be called biological decrepitude. It has been asserted that man, the highest race of living creatures which the earth has yet known, is not only the highest she will ever know, but that the race, regarded as a type of animal life, has already passed its prime, and has advanced perceptibly towards decadence.[1] Lower races, however, seem capable almost of indefinite multiplication—we refer, be it understood, to the multiplication of races, not of the individuals composing races. And, so far as mere life is concerned, it would seem as though the earth might undergo vast changes of condition, and the sun himself lose largely in heat-emitting and light-emitting power, without the earth being depopulated, so long at least as the changes took place gradually. It may well be that life begins at so early a stage of planetary development, and continues to so late a stage, that the entire duration of a planet's life-bearing era bears a much greater proportion to the entire duration of the planet than our reasoning (a few paragraphs back) implies.

But, after all, the question of mere life in other worlds is not what we are interested in. Mere consciousness can scarcely be regarded as a more interesting phase of nature than unconscious activity such as we see in the vegetable world, or than the motion of inert matter, or even than the mere existence of matter. If we could be assured that Mars and Venus and

[2] One of the evidences for this discouraging conclusion advanced by a well-known American zoologist, is the relative length of the period of old age in the individual man. In youthful races, the individual does not attain old age till very soon (relatively to the entire life) before death. The relative duration of old age grows longer and longer as the race grows older, until, in races which are about to pass away, it becomes nearly equal to half the entire interval between birth and death, soon after which the race dies out.

Mercury are crowded with animal and vegetable life of those lower forms which owe their inferiority to decrepitude of the type, or that on the youthful planets Jupiter and Saturn some of the monstrous forms exist which flourished on the earth when she was young—

> Dragons of the prime
> That tare each other in their slime—

what to us would be those teeming worlds of life? They might as well be mere inert masses circling idly round the sun, neither now nor ever in the past the abode of life, and never to become so in future ages. The story of such life would be to us as

> A tale
> Told by an idiot, full of sound and fury
> Signifying nothing.

It is the existence of intelligent beings on those remote worlds that alone has any interest for us, the thought that the wonders of the universe are recognised by beings in some sort like ourselves, that the problems which perplex us may have been dealt with, perchance even solved, by others, and again that our world may be a subject of interest and study for creatures thinking as much, but knowing as little, about us as we think and know about them.

In this respect certainly, if analogy can be any guide at all, we find little reason for regarding with present interest either the younger giant planets, Jupiter and Saturn, or the probably aged dwarfs, Mercury, Mars, and our moon. Few believe that men have existed on the earth many hundreds of thousands of years, and those even who assign to the human race its greatest duration in the past regard it in its earliest form as little better than a race of brute beings. If we suppose that men sufficiently intelligent to consider the heavens and the earth have existed in our world for one hundred thousand years, we are certainly giving the widest possible allowance of duration to intelligent man. Nor can it be denied that the existence of such a race as ours seems far more definitely limited in the future by the slowly changing condition of our earth and the life-giving sun, than that of lower types of animal existence. We would not assert that beyond all question a hundred thousand years hence the earth will no longer be a fit abode for man, who has already begun to draw very largely on the garnered stores of our globe; but we consider this view altogether probable, and that indeed a nearer limit might be assigned to the duration of the human race by one who should carefully consider the progress and requirements of the race on the one hand, and the condition, changes of condition, and capabilities of our earth, on the other.

If we assign two hundred thousand years as the extreme duration of the period during which men capable of observing the phenomena surrounding them and of studying the problems of the universe have existed and will exist, we assign to our earth a reason-life (if we may so speak) which, compared with the full life of the earth, is but as a second compared with centuries. So far as the existence of beings capable of thought and reflection is concerned, our theory assuredly holds. It is, on *à priori* grounds, utterly unlikely that any one of the orbs we can actually observe is inhabited by creatures like ourselves in those circumstances which distinguish us from the brutes and from savages.

So far as observation extends, in the case of Mars, it seems altogether unlikely that the present era of his existence corresponds with that very brief period during which reasoning creatures inhabit a planet. Supposing we have rightly taken two hundred thousand years for the duration of that period in our earth's case—and it seems far more likely that the estimate errs in excess than in defect—the duration of the corresponding period in the case of Mars would probably be about 70,000 years. Mars would probably have entered on that stage of his existence millions of years ago; but supposing for a moment that he reached it at about the same time as our earth, or, according to our estimate, a hundred thousand years ago, then the period would have been completed about 30,000 years ago. The appearance of the planet implies a much later stage, however, of planetary existence. The seas of Mars present all the appearance of exhaustion during millions of years, in the course of which their waters have nourished the surface of the planet with rain. The water thus raised from the Martian oceans has no doubt been always restored to them in large part, either falling directly on the water surface in rain, or being gathered by streams and rivulets and rivers on the land surface, to be discharged by the river mouths into the seas. But a portion has always been retained by the land, soaking slowly and steadily into the interior of the planet. This portion has doubtless been exceedingly small each year, but during the long ages which have elapsed since first the seas of Mars had separate existence, the total amount thus drained off must have been enormous. We see the effect in the relatively small area of the Martian seas. They cover barely half of the planet, while terrestrial seas occupy nearly three-fourths of the surface of our globe. They have the shape also which our seas would have if somewhat more than two-thirds of the water were dried up. The variety of tint which they present shows that but few of those seas are deep, for few of them are dark. Many are so light as to suggest the idea that a large part of the area shown in the charts as aqueous consists in reality of land and water so broken up into small islands, lakes, straits, isthmuses, and the rest, that the telescope cannot distinguish the details. Again, the unchanging colour of the land regions implies that they are naked and sterile. Unless we adopt the theory that not only is the vegetation of Mars rubescent, but that all the principal glories of the Martian forests are ever-reds, and the Martian fields covered with herbage of unchanging ruddiness, we must accept the conclusion that the land surface is an arid desert. This evidence alone is almost strong enough to assure us that none but the lowest forms of life, animal and vegetable, exist on Mars at present. The evidence against the fitness of Mars to support the higher forms of life seems overwhelmingly strong.

But, after all, why should a conclusion such as this dishearten the student of other worlds than ours? Whether it relates to a planet here and there, to Mars or Mercury or the Moon because of their decrepitude, or to Jupiter and Saturn because they are as yet too young, or whether it is extended, according to the laws of probability, to the universe of planets, does it not accord with what we know of our own earth? We do not mean merely that our earth as a planet was once unfit, and will one day become again unfit, to support life; but that even during the present life-supporting era of its existence we do not find all regions of the earth at all times fit to support life; nor do we find all races existing simultaneously. As various races begin, develope, and die out, as various regions are at one time sterile, at another clothed with life, so among the orbs inhabiting space, now one set of races may exist and anon an entirely different set, the series of planets which during one era are the abode of life being the nascent worlds of a former, the dead worlds of a later era. A modern believer in the universality of life says: 'On those worlds, as on ours, there are cities passing through all the stages of glory and of power; there also, as here, there are cities like Rome, and Paris, and London, altars and thrones, temples and palaces, wealth and misery, splendours and ruins. And perchance from the venerable ruins of an ancient capital two lovers at this moment on the planet Mars may be gazing on the traces of the grandeur and of the decay of empires, and feeling that amid all the metamorphoses of time and space, life, eternally young, pervades the universe, reigning for ever over all the worlds, and pouring forth endless youth in the golden rays of all the suns which people infinity.' But the very scene which suggested these ideas should have taught another lesson. Not every region of earth is inhabited, not every inhabited region is a Rome, or an Athens, or a Paris, or a London. While some great nation or city is enjoying the fulness of its vigour, others are perishing or have long since passed away, others are as yet unknown, or but begin their existence. So may it well be, so *must* it be if analogy is our guide, so *is* it if our observations can be trusted, with other planets than this earth, with other systems than our sun's. As each orb occupies but the minutest portion of the infinity of space, so is the lifetime of each but a wavelet in the ocean of eternity. Two wavelets, or many, may run side by side upon an endless sea, and so may the lifetime of our earth synchronise with life upon another world, or many others. But for each wave that thus runs beside the wave of life on which our lot is cast, a myriad—nay, ten million million others—are far removed from ours, lie even beyond the horizon bounding what we call time. The universe as we know it, the region of space to which our most powerful telescopes penetrate, is not more utterly lost in the true universe of infinity than is the range of time past, present, and to come, over which our researches extend, amid the infinities of time eternal.

LIVING IN DREAD AND TERROR.

I MAY as well say at the outset that I have no personal experiences to relate. Students of science may ere now have lived in dread and terror, though I know of no instances of the sort. Usually the life scientific is a life of peace and quiet, remote from war's alarms. Yet it is of Dread and Terror as the attendants of War that I am now to speak. For I cannot doubt that the excellent suggestion made by Mr. Madan of Eton, that the two moons of the War-planet should be called Deimus and Phobus (or Dread and Terror), will be adopted by astronomers, and the comparatively unmeaning names Romulus and Remus (suggested in an American journal) rejected in their favour. Every reader of the 'Iliad' will remember the description, in the fifteenth book, of the wrath of Mars when his son Ascalaphus was slain. Forgive me, Gods, he says,

> and yield my vengeance way:
> Descending first to yon forbidden plain,
> The God of Battles dares avenge the slain;
> Dares, though the thunder bursting o'er my head
> Should hurl me blazing on those heaps of dead.

With these rebellious words, he calls on his two satellites (or, according to others, his two sons) Deimus and Phobus :—

With that he gives command to Fear and Flight
To join his rapid coursers for the fight;
Then, grim in arms, with hasty vengeance flies—
Arms that reflect a radiance through the skies.

Or, as Homer more succinctly expresses his meaning: 'Thus spake he, and bade Fear and Flight his coursers yoke, while he donned his blazing panoply.'

Mr. Madan suggests, doubtless in jest, that the preparation of Ares to descend to the earth possibly refers to an unusually near approach of the planet at opposition. But the gods of the Homeric Olympus are not planets;[1] and we may be permitted to regard this

[1] I myself once threw out the notion, however, that there was a reference to the immense superiority of Jupiter's mass in the well-known lines in which Zeus reminds the other deities how much stronger he is than they are. Remembering that Jupiter's mass would outweigh about two and a half times the combined mass of all the other planets, a fanciful imagination might find a reference to this relation in the words which are rendered (considerably 'translated' in the sense in which Quince used the words) by Pope:—

'League all your forces then, ye powers above,
Join all and try th' omnipotence of Jove;
Let down our golden everlasting chain,
Whose strong embrace holds heaven, and earth, and main:'

(what can this be but universal gravitation?)

'Strive all, of mortal and immortal birth,
To drag by this the Thunderer down to earth:
Ye strive in vain! If I but stretch this hand,
I heave the gods, the ocean, and the land;
I fix the chain to great Olympus' height,
And the vast world hangs trembling in my sight!'

I remarked on this, in Part A of the appendix to my 'Saturn and its System,' that possibly some tradition of the vastness of Jupiter's bulk, ascertained perhaps by Chaldæan astronomers, may have been embodied by Homer in the above passage, or rather in the speculation as on the whole improbable, especially as what immediately follows is hardly consistent with any planetary interpretation. For Pallas snatches from Mars his shield and spear, and, taking off his helmet, addresses him in terms of strong reproach and menace, in such sort as not only to prevent his approaching the earth, but to cause Ares the Manslayer, Shaker of Cities, to sit in sullen gloom curbing the rising groan. The astronomer, then. can hardly encourage the hope that any nearer approach may be made to the date of the Trojan War by this reference to Mars, or accept quite unhesitatingly the belief that the prophetic genius of Homer has already not only identified but even given names to the two minute moons detected by Professor Asaph Hall (with a telescope which not ten such men as fought at Troy could have moved, but Hall or Newcomb now easily wield it in whatever direction they may desire). Yet, as Mr. Madan says, we must not lightly reject the authority of Homer, and the names Deimus and Phobus, or Dread and Terror, will most probably be accepted gladly by the astronomers of the Washington Observatory, with whom unquestionably rests the decision of this interesting question. Should other Martian satellites hereafter be

Greek original, which differs considerably in places from Pope's rendering. 'The image employed seems singularly infelicitous,' I wrote, 'unless interpreted in some such way'—an explanation which 'appears more natural than that commonly offered, which refers the image to subtle dogmas of physical influences and powers, associating together the various parts of the universe.' I must confess, however, I do not now see this quite so clearly as I did, or thought I did, when I wrote the above remarks.

detected, which seems not improbable, when we remember that other moons of the same order in point of size might readily have escaped detection even with the great 26-feet lens or with the mightiest mirrors yet made, it will not be difficult to find names for them. Our poet Gray, following Homer in assigning as a warrior's attendants Dread and Terror (regarding Dread as the sentiment of fear, and Terror as its outward expression, so that Amazement and Flight become no unsatisfactory synonyms), gives us Solitude and Sorrow for followers, in the familiar lines—

> Amazement in his van with Flight combined,
> And Sorrow's faded form and Solitude behind.

Fainter and probably smaller moons of Mars may have these names, and if yet others should be detected, the melancholy train may be further increased without difficulty. If Romulus and Remus were selected, a difficulty would arise in assigning names to other Martian moons, for we do not know of any brothers which these twins had (though they had half-brothers in plenty). Besides, they were twins, and their names would suit only a binary system, not two moons travelling on different orbits and in widely different periods. (Romulus and Remus were in the strictest sense men of the same period, though Romulus flourished later than Remus for a very sufficient reason.)

It is, then, of life in the moons of Mars that I propose to speak. The subject is one with which I feel particularly free to deal, simply because I know nothing whatever of life in these moons, and where we know nothing surmise is free from restraint. It is indeed only where we know nothing that the old proverb ‘Thought is free’ is true, as all proverbs should be.

I feel a little difficulty, nevertheless, at the outset in determining what sort of life I shall consider in these Martian moons. I do not mean that I am in doubt whether animal or vegetable life, or both forms of life, are best suited to these bodies, or whether in considering animal life I shall take it for granted that some animals there possess intelligence resembling that of man. I am quite decided on these points. It seems to me, indeed, that there is very little interest in the inquiry whether vegetables or brute animals exist anywhere else in the universe save in our own earth. The mere repetition in unnumbered worlds of the forms of a vegetable and animal life (other than man) existing on this earth, seems to me a matter of no greater moment than the multiplication of worlds like our earth, or systems like our solar system, of galaxies like the universe of suns of which our sun is a member. Granting only such kinds of life elsewhere, we might use a formula somewhat resembling one used by an eminent American divine at an inaugural meeting held in New York before I gave my first series of lectures there. The poet Bryant had spoken of the wealth of worlds throughout the universe, their stately motions, the splendour and the multitude of suns, the glories, in fine, of the universe of worlds and suns. Presently Dr. Hitchcock arose and said that, while recognising the vastness of creation in these respects, he felt that a universe not 'informed with life' taught only, however splendid it might be, however noble its proportions, however grand its movements, the unimpressive doctrine that 'Dirt is cheap.' Regarding dirt —not, after Palmerston's definition, as matter in the wrong place—but as representing mere inert matter, the saying is just, and may be justly matched by the saying, 'Brute life is cheap,' if we assume that such life is common to all or most of the worlds which people space; and remembering what brute life really is, how it is only maintained by constant death, and ends in death even when so maintained, may we not fairly substitute for the 'Brute life is cheap,' the formula 'Death is cheap'? Unquestionably dirt is cheap throughout the universe, and if the general theory of the plurality of worlds is accepted, assuredly death also is cheap. In the mere existence of life no greater interest can rationally be taken than in the motions, shape, qualities, and so forth, of inert masses. Each subject alike interests rather as affording scope for human intelligence; but even in that respect such subjects derive no small proportion of their interest from their relation to the possible existence of intelligent beings in the worlds whose masses, motions, physical properties, and so forth, the intelligence of man has enabled him in some degree to determine.

It is not, therefore, in this respect that I am in any doubt. I decide unhesitatingly to consider the circumstances under which beings possessing intelligence like ours would find themselves placed in such abodes as the moons of Mars. But I feel in some degree of doubt as to the bodily attributes to be assigned to such beings. The consideration of the cause of my difficulty in this respect will introduce us conveniently to some of the general relations involved in our subject.

I entertain, for my own part, the conviction that the outer moon of Mars cannot be much more than ten miles in diameter, nor the inner moon much more than fifteen miles. The opinion is founded on the apparent lustre of these two objects, for no attempt to measure them will be made until telescopes many times more powerful than any now in existence shall have been constructed. In passing I may remark that a statement made in a scientific weekly to the effect that the great Parsonstown reflector could have been employed on a special occasion to measure the satellites has been misinterpreted by persons not familiar with the technical terms in use among astronomers. By measuring the satellites the observers who used the expression meant only measuring the apparent distance of the satellites from Mars, and their apparent position with respect to him, not measuring their dimensions. I doubt if we shall know anything on this point for many years to come, if even for many centuries. But we can form a fair estimate of their size from the quantity of light we receive from them. I will not here run through the calculation by which I first showed that the outermost moon cannot probably exceed ten miles in diameter. It is contained in an article which

I wrote for the 'Spectator' (September 1), and is given more fully in an essay of mine which appeared in the 'Cornhill Magazine' for September. It was confirmed, within a week or so, by the news that Professor Newcomb, of the Washington Observatory, considered the diameter of the outer satellite cannot greatly exceed ten miles, and may be less. The only reason for doubting this conclusion is that Mr. Wentworth Erck, of Sherrington Bray, has seen the satellite with a telescope only seven inches in aperture—a wonderful feat of eyesight. It would have led to the suggestion that a faint star had been mistaken for the satellite, but that the correct place and change of place were given by Mr. Erck. However, I am disposed to regard the achievement as altogether exceptional and affording (in the presence of the failure of other observers with tenfold greater illuminating power) no better reason for regarding the moons of Mars as within the range of such telescopes, than the success of Mr. Ward of Belfast in seeing two of the Uranian satellites with four inches only of aperture affords for believing that they can ordinarily be seen with anything like that telescopic power.

However, let us assign to both the moons a diameter of about 20 miles, which (not to trouble the reader with exact calculations) we shall regard according to convenience as equivalent to the 400th part of our earth's diameter, or the 100th part of the moon's diameter. Thus the moons of Mars would have a surface equal to the 160,000th of the earth's, or the

10,000th of the moon's, and a volume equal to the 64,000,000th of the earth's, or the 1,000,000th of the moon's. As to their mass we are driven again to a mere assumption, for we know nothing of the density of these moons. We may fairly assume that their substance is not so compact as our earth's, for we know that our own moon and the moons of Jupiter are of smaller mean density than the earth. The reason of this is presumably that, owing to the relatively smaller quantity of matter in our moon and Jupiter's moons as compared with the earth, the matter is not so forcibly compressed by gravity. We might even assume on this account that the moons of Mars were much less densely compacted even than our own moon; but we must remember, on the other side, the possibility that both moons might be masses of meteoric metal. I do not myself regard this as at all probable. I feel satisfied, from the known facts respecting meteors, that none of them travel as metallic masses in space, although, after the changes they undergo in their rush through our air, metallic masses only may often remain as visible evidence of their visit. But the possibility must be taken into account. I think the assumption that these moons have a mean density not exceeding our moon's will be regarded as a fair one.

Now, on this assumption, it will follow, according to a well-known law, that the attraction of gravity at the surface of one of these moons will be less than the attraction at our moon's surface, in the same degree that the diameter of a Martian moon falls short of our moon's diameter. It will therefore be only one hundredth part of the attraction at our moon's surface, or about one six-hundredth part of our familiar terrestrial gravity. So that if a man 10st. 10lbs. (or 150 lbs.) in weight were placed on the surface of one of these moons he would weigh only a quarter of a pound (so that if he were, as Sydney Smith said of Macaulay, 'a book in breeches,' he could be forwarded by book-post for a penny, assuming size to be no objection).

It is here that my difficulty comes in. If I assume the intelligent beings inhabiting the moons of Mars to be shaped like men and of the same size, while circumstances are so arranged that in nervous and muscular energy they are not inferior to us, then their home would be inconveniently small for them, because of their amazing activity. For with the same muscular power and size as at present, but weight reduced to a six-hundredth part, or to a few ounces, an active man could leap to a height of about 3,000 feet, or to a distance of 12,000 feet; that is, to a height of more than half a mile, and to a distance of more than two miles.

Here I have simply multiplied the greatest height and the greatest distance which an exceptionally active man can jump, by 600. But there is another way of considering the high jump, and to some degree the long jump also,—because in taking a long jump a man must give a certain vertical impulse to his body as well as the horizontal impulse which gives range to his leap. When a man jumps to a height of $5\frac{1}{4}$ feet, he gives to his body a vertical velocity upwards equivalent to that with which a body reaches the ground after falling through about 4 feet (not $5\frac{1}{4}$, because part of the range in height is due to the drawing up of the lower limbs; in fact, I question whether the best jumpers raise the centre of gravity of the body by so much as 4 feet, even in taking an upward leap of $5\frac{3}{4}$ or 6 feet, but exact accuracy is not very important). This would correspond to a velocity of about 16 feet per second. Now, we might at first sight assume that on one of the Martian moons an active man could communicate to his body in springing upwards a velocity 600 times as great, because he would apply the same total impulse to a weight of 4 ounces that a man on earth applies to 150 lbs. when he springs upwards. He would leap upwards then with a velocity of 9,600 feet per second, or not very far short of two miles per second. But the muscles cannot under any conditions move at this rate. Sensation itself only traverses the nervous system at the rate of about 120 feet per second, under the most favourable conditions.[1] Besides, it need hardly be said that impelling the body from the ground at the rate of two miles per second would be precisely as injurious to the body (whether on our earth or on a Martian moon) as flinging it against the ground with this velocity, or 120 times that of an express train. I will not even ask that the upward velocity given to the body by an active jumper on a Martian moon should be that acquired in falling on the earth

[1] Only 17 feet per second in some cases of comparatively slow sensation.

from a height of 16 feet, about as much as even the most lissom and active person could safely endure. I will simply ask that the velocity may be that corresponding to a terrestrial leap or to a fall of 4 feet—a velocity, namely, of 16 feet per second.

Suppose then a man like one of us to leap from the surface of one of the Martian moons with a velocity of 16 feet per second, and let us consider what would be the nature of the resulting jump. He would continue to travel upwards during the time required by gravity on the Martian moon to destroy this velocity of 16 feet per second. Gravity on earth would do this in half a second; but gravity on the Martian moon would take 600 times as long, or five minutes. This is the time our gymnast would be rising, and he would be as long falling again, or ten minutes in all in the air. The height to which he would rise is that through which a body would fall in five minutes under the action of the Martian moon's gravity; and this is readily found to be 2,400 feet, or nearly half a mile. This is less than our former result, but still a very good jump.

It might readily be shown that the long leap of such a gymnast would not, if treated in this way, be less than our former result. For a man in leaping gives to his body a certain horizontal velocity which, if he did not reach the ground when he does, would carry him onwards with almost unchanged horizontal rate until he did reach the ground (for instance, if a man leapt with powerful horizontal impulse from the summit of a lofty perpendicular wall, the spot where he would reach the ground would be farther from the wall the higher the wall might be, in direct proportion in fact to the time he was in the air), so that the range of the leap depends on the time during which the body is in the air, as well as on the velocity given to the body horizontally. A leap which on earth would give a horizontal range of 20 feet, and a stay in the air of half a second in all, would in Mars give a stay in the air lasting 300 seconds, and a range of 600 times 20 feet, or of 12,000 feet.

But it would manifestly be absurd for beings living in a world whose diameter is but about 20 miles to be able to leap to a height of nearly half a mile; or, where the circumference is but 63 miles, to be able to leap a distance of $2\frac{1}{4}$ miles. Comparing the Martian moon-men with a gymnast taking a run of twenty steps, each 4 feet long, and then leaping 20 feet, we see that for a similar effort on the Martian moon a run of nine miles, followed by a leap of $2\frac{1}{4}$ miles, or in all about a sixth part of the entire circuit of his world, would be required. This corresponds to the distance between England and India or mid Africa or the middle of the United States, or from Scotland over the North Pole to Behring's Straits; and cannot be thought of for a moment.

For a run corresponding to a half-mile spin on our earth—that is, requiring the same amount of exertion—it would be necessary to travel nearly five times round one of these Martian moons. More time would certainly be required, which would however make the effort lighter. But at each spring from the earth, a distance would be traversed exceeding six-hundredfold that traversed by a terrestrial athlete urging his body forward at each spring with similar velocity. Thus the distance traversed would be 300 miles, or nearly five times the circuit of the satellite. The time taken in the journey would also be 600 times greater; so that if we assume $2\frac{1}{2}$ minutes to be a fair time for running half a mile on earth (it is often done in less than 2 minutes), the athlete on the Martian moon would complete the journey five times round his world in 1,500 minutes, or in one day and one hour. Supposing the day in his world to last as long as our day, and that he allowed himself four hours during the day for rest and meals, so that he made four circuits a day, then, if he started at sunrise and ran westwards, the sun would immediately set in the east, owing to the athlete's more rapid westerly motion. He would continue for a while running through a reversed night till the sun would rise in the west. Then the athlete would run through a reversed day, the sun setting in the east, and so on; till at the end of the twenty-four hours he would complete his fourth circuit, and find the sun again rising on the eastern horizon, about to set *for him* if he continued his westward journey, but about to rise for his fellow-residents in the Martian moon. So that if he rested at the end of his long journey, the sun, which during the latter portion of it had passed from the western horizon athwart the sky to the eastern, would, after the athlete stopped, rise from the eastern horizon and pass back again to the western. Thus he would have, during and after the latter part of his journey, two days in succession, one day reversed as he ran westwards, the other the natural day of the Martian moon. If, on the contrary, he started at sunset, he would begin his journey with a reversed day, and close it with a reversed night, followed by an ordinary night, during which he might rest. It would also be easy for him so to arrange matters, by running alternately east and west, as to have daylight throughout his journey, though to Martian lunarians at rest four days and four nights would have passed.

But a world admitting of such vagaries as these is not to be readily accepted as a reasonable or probable world. On the other hand, if we assign to the inhabitants of Martian moons proportions which would prevent their leaping to such enormous heights and over such enormous distances, or rather to heights and over distances so many times exceeding their own height, we find ourselves surrounded by perplexities not a whit less confounding than those just considered.

It is easy to determine how large a man should be, in order that on the surface of a planet whose gravity is known he might be as agile as a man on our earth. For this purpose we must compare, first, the relative activity of two men of unequal size but similarly proportioned. When I speak of activity here, I mean the relation between the strength of the limbs and the weight of the body, regarding two men as equally active if the strength of the heavier exceeds that of the lighter in the same degree that the weight of the former exceeds that of the latter. Now compare two men in these respects whose heights are five feet and six feet respectively, their proportions being in all respects similar. The strength of muscles depends on the size of their cross-sections, and these are not proportioned to the height but to the square of the height. Thus the muscular power of the larger man is to that of the smaller, not as 6 to 5, but as 36 to 25, or exceeds that of the shorter man in a much greater degree than the height does. But the weight of the larger man exceeds that of the smaller in a yet greater degree, being greater, not as 6 to 5, or as 36 to 25, but as 216 to 125, or very nearly double. His muscular power being greater only in the proportion of 36 to 25, whereas it should be greater in the proportion of 216 to 125, is less effective to support the weight in the proportion of 5 to 6. We see this at once, because if it were increased as 6 to 5, it would be greater than that of the smaller man as 36 times 6 or 216 is greater than 25 times 5 or 125—that is, in the same degree as the weight is greater. Since then the strength of the taller man should be increased as 6 to 5, to make his activity equal to that of the smaller, it is in reality too small in the proportion of 5 to 6. In other words, the smaller man is more active than the larger in the same degree that the larger is taller than the smaller. But if the larger lived in a world where gravity was only five-sixths of terrestrial gravity, he would be as active in that world as the smaller man in *his* world.

The taller man, then, wants a world where gravity is less in the same degree; and conversely, in a world where gravity is less, men can be larger in the same degree, yet remain equally active as we are in ours. Applying this reasoning to either of the moons of Mars whose gravity we have assumed equal to a six-hundredth only of terrestrial gravity, we arrive at the stupendous—the appalling—result, that men there might be six hundred times as tall as terrestrial men, yet equally active. The same reasoning applies to animals, and the idea of an elephant or a giraffe six hundred times as tall as terrestrial specimens of these animals is dreadful indeed. But let us content ourselves with considering human beings only. The Brobdingnags of Swift sink into utter insignificance beside giants 1,200 yards high. The average height of a Brobdingnag was about 20 yards, or ten times the height of ordinary men. So that the inhabitants of a Martian moon, on the assumption we have been dealing with, would exceed a Brobdingnag sixty times in height, or six times more than a Brobdingnag exceeded Gulliver, or than Gulliver exceeded the King of Lilliput in height. Amongst the Martian lunarians a Brobdingnag would be almost as utterly insignificant as a Lilliputian among Brobdingnags.

But this arrangement, though reducing the Martian lunarians to a reasonable degree of activity, so that the most agile among them would only be able to leap to about his own height, or over a distance exceeding his own height about fourfold, only increases our difficulties. The rate at which these monsters would travel, supposing they used as much exertion as terrestrial men in walking and running, would in fact be much greater,—for while every step would be six hundred times as long as before, the giant would not be six hundred times as long making each step, as we before supposed. What time he would actually require I do not know, having had no experience of the habitudes of human bodies 1,200 yards high. We cannot suppose he would take a step in the same time as a man on this earth, simply because the nervous system would require a considerable time in conveying sensations to the monster's brain—about half a minute, for example, in conveying intelligence from the foot to the brain. But doubtless each step would take much less than six hundred times the interval required for terrestrial steps. So that the pace at which these giants would travel would far exceed that at which the smaller lunarians we before considered would advance. Each giant would also require much more room for himself in taking his journeys; and in fact the small moons of Mars would be utterly unfit abodes for such creatures. One circumstance alone, among many suggested in the preceding inquiry, will show how utterly uncomfortable the relations of such a world would be. Sensation requiring half a minute to travel from the foot to the brain of one of the giant inhabitants, it follows that some one might deal a blow at his enemy's foot, and have a clear start of one minute before the giant could pursue him,—half a minute passing before the wounded giant knew he was hurt, and another half-minute before he could set his feet in motion to pursue his foe.

Probably the most convenient assumption we can make, under these circumstances, is that there may be creatures in a general respect like ourselves on these moons of Mars, but that, owing to the extreme rarity of the atmosphere, their vital energy is so far reduced that they are not more active than we are, despite the feeble action of gravity in their world. The air must be exceedingly rare, most certainly, even if the quantity is proportioned to the volumes of these moons. On this assumption the quantity of air is less than the quantity of terrestrial air as one is less than 400 times 400 times 400,—that is, amounts only to one sixty-four millionth part of the terrestrial air. Being spread over a surface which is but one 160,000th of the earth's, it follows that the quantity of air above each square mile of surface is one 400th part only of the quantity over each square mile of our earth's surface. This would be little enough in all conscience; but this is not all. For the action of gravity being, according to our assumption, only one 600th of terrestrial gravity, it follows that the atmospheric pressure and therefore density is further reduced in this degree, giving finally a density equal only to one 240,000th of the density of our own air. Now at a height of seven miles, where the atmospheric pressure is reduced to one-fourth that at the sea level, men of ordinary constitution would perish in a few minutes, if not instantly. In Coxwell's ascent to nearly that height, Glaisher fainted, and

Coxwell only just had strength left to draw the valve-string with his teeth (his hands being already powerless). Yet at a height of seven miles, the density of the air is 60,000 times greater than that which, according to our very reasonable assumption, prevails at the surface of the Martian moons. We can very well believe, then, that, in whatever way the inhabitants of these moons may be adapted, corporeally and constitutionally, for existence in their small homes, the rarity of the air there must tend to reduce their vital energy. So that we may well imagine that instead of being able to leap to a height of half a mile, or over a distance of two or three miles, they are not more active than we are on earth with six hundred times greater weight, but a far more effective respiration.

We might perhaps go even farther than this, and assume that, in order to give to the inhabitants of these moons locomotive powers proportioned in the same way to their own dimensions as ours are, they must be supposed very much smaller than we are. We might imagine them in an atmosphere so exceedingly attenuated that creatures which could have vitality enough to move freely about must be no larger than flies or ants, and must have also some such provision as insects have for more effective respiration. In this way we might find in the Martian moons a miniature of our own earth, not only in the proportions of these worlds themselves, their lands and seas and atmospheres, but also in those of the creatures living upon them. But it would not be very interesting to consider mere miniatures of our earth such as the moons of Mars would thus come to be regarded. Indeed, in that case, little more could be said than that all the relations presented by this earth were, or might be, represented in the Martian moons, but on a greatly reduced scale.

It will be much more interesting to imagine beings like ourselves, except in the possession of respiratory organs enabling them to live in an exceedingly rare atmosphere, and also in possessing only so much vital energy as enables them, though only weighing a few ounces, to travel about as actively in their home as we do in ours.

We may then try to picture to ourselves our condition if we lived in a world twenty miles in diameter, and situated like either of the moons of Mars.

Such a world would have a surface of about 1,260 square miles, about the area of Suffolk. As has been pointed out recently in a very suggestive and interesting article in the 'Spectator' a certain allowance must be made for seas. Let us assign to water one half of the surface, so that there are 630 square miles of water surface and as many of land. Thus the water surface would be equal to about four times that of Lake Neagh, the land surface to about that of Hertfordshire. Whether we ought to regard any portions around the poles as uninhabitable by reason of excessive cold may be questioned. For, as was pointed out in the same essay, the smallness of the moon-world's surface could hardly fail to have its effect in rendering the climate more uniform than it would otherwise be.

If we imagine arctic and antarctic snows around the northern and southern poles, then, as little more than thirty miles separate the poles, and perhaps only twenty miles would intervene between the outer edges of the snow regions, the state of things would correspond to the existence of two very large ice-houses (shielded carefully from the sun) at a distance of about thirty miles from each other in some well-watered country in our temperate zones. As we know that artificial measures would be necessary to prevent the ice so collected from melting entirely away in the course of time, we can hardly suppose that ice and snow can have gathered in the imagined circumstances around the poles of the two Martian moons. However, we may well suppose that the polar regions would not be very comfortable places to live in. Probably the inhabitable area of each moon is reduced by some eighty or a hundred square miles in this way.

But the smallness of these moons would render the temperature much more uniform. I fear the rarity of the air introduces here a difficulty which, if we were to attend closely to it, would compel us to abandon altogether the idea that the kind of life we have been imagining exists at all in these moons. At any rate we could only get over the difficulty, I think, by assuming that some peculiarity of bodily structure and of constitution enabled the human inhabitants of these worlds to endure an intensity of cold which *we* should find as quickly fatal as the small atmospheric density. Let us pass over this difficulty, however, and suppose that, utterly different as the mean temperature must be from that of our earth, the varieties of temperature in different latitudes and in different seasons correspond, as on our earth, with the varying solar elevation.

A degree of longitude at the equator would correspond to a distance of about one-sixth of a mile, instead of some 69 miles as on our earth. So that, as fifteen degrees or $2\frac{1}{2}$ miles would correspond with a time difference of one hour, it would be very easy for an inhabitant by walking westwards to make the sun seem always in the same part of the sky. Say, for instance, it was noon, and therefore the sun due south. Then if he walked at the rate of $2\frac{1}{2}$ miles westwards along the equator it would remain noon just as long as he chose to continue his journey. A walker as good as Weston could quite easily keep the sun within a few degrees of south for a week or a month at a time. If he walked at the rate of 5 miles an hour westwards, the sun would seem to go backwards, or towards the east, and at the end of two hours, supposing it had been due south when he began, would have passed backwards by two hours' motion or fifteen degrees. Then he might rest for four hours, finding the sun two hours' motion west of south. Two hours' walking would bring the sun back to the south, two hours more would bring the sun an hour's motion east of south, and then he could rest again for four hours. Walking thus half the time, in spells of four hours, with four hours' rest between them, he would keep the sun always within two hours' motion on either side of due south—or, in other words, it would be always between ten in the morning and two in the afternoon with him.

In a latitude corresponding to that of London the task would be much easier, for the distance to be travelled per day would be reduced from about 63 miles to less than 40 miles.

Travelling eastwards, the sun's motion would be hastened instead of retarded or reversed. If, for instance, a traveller at the equator walked eastwards at the rate of 2½ miles an hour, the sun's apparent westwardly motion would be doubled, and daylight would last only about six hours, night the same, if he continued walking at this rate from one morning to the next, or for twelve hours. If he walked at the rate of 5 miles an hour, day would last only four hours, and the interval from dawn to dawn only eight.

Means of locomotion such as we have on earth would produce still more remarkable changes; but I need not consider them. The reader will have no difficulty in perceiving what their general nature would be. It would be quite easy, in certain latitudes, to reduce the day to a single hour either by swift journeying eastwards or by swift journeying westwards. In the former case the sun would rise in the east and set in the west. In the latter it would rise in the west and set in the east, and he would require to travel somewhat more quickly than in the former case. It is not easy to see how any form of steam mechanism could be devised where atmospheric pressure would be so small. Water would evaporate at a very low temperature indeed.

But the most remarkable effects which change of place by ordinary methods of locomotion would produce are those due to journeys north and south. As the writer of the interesting article in the 'Spectator' already referred to has pointed out, 'in a 3-mile walk the inhabitant of one of these moons might change his climate as vastly as an inhabitant of our earth who has sailed from London to Alexandria. In other words, the same change which can only be obtained in the same distance on our earth by climbing a high mountain, could be obtained by an easy walk on the surface of the Martian moon; while a railway journey of the same distance which takes a man from Richmond or Twickenham to town, might, on a little world of these dimensions, take him from the temperate to the Arctic or the Torrid Zone. Of course it is difficult to say how far mere minuteness of scale might not alter essentially the rules of temperature. It is difficult to suppose that mere contiguity would not itself prevent the great differences of temperature which are possible on a larger globe. Granting even an exact parallelism in the causes at work, and no difference except the reduction of scale, you could not have as many zones of temperature in a pond four feet deep as you may have in the Atlantic or the Pacific, if only because heat is conducted much more rapidly, and is much sooner equalised, over a small surface than over a large. But making all proper allowances for this, any difference in temperature solely due to the angle at which the sun's rays strike down would be as great after a seven or eight miles' journey, north or south, in such a tiny world as this, as it would be on our own earth after a journey of 3,000 miles.'

I do not myself care to examine very closely the effects of changes of latitude in our imagined small world, simply because, on making an approach even to a close scrutiny of the conditions of life in such a world, a host of reasons present themselves against even the possibility of creatures existing there resembling man in the remotest degree. It spoils the pleasing illusion by which alone one is enabled to speak of the experiences of beings like ourselves in such a world, to inquire closely into their surroundings.

We are able somewhat more safely to consider the relations of inert matter in worlds where gravity is so small as it must be in the Martian moons. It has been said, by myself amongst others, that building, and all engineering processes, would be much easier in a world where gravity is very small than in our own world. But if less arduous, so far as mere labour is concerned, they would in some respects be far more difficult. Much more skill would be required to give adequate stability to buildings, made even of the most solid materials, in a world where all weights are so much reduced. Suppose even platinum were available in sufficient quantities for architectural purposes, yet a block of platinum of given size would press downwards with less force there than a block of deal wood of about one-twentieth its size on our earth. Whewell has well described in his Bridgewater Treatise the effects of a great reduction in the force of gravity, though he is not there considering life in other worlds, but the importance (which many are apt to overlook) of those portions of our earth's frame which lie far below the deepest mines even yet dug by man. If the interior of our earth could be scooped out and removed, we should in fact suffer from precisely the same inconveniences as would affect creatures like ourselves living in the moons of Mars. We should indeed find ourselves living like them in dread and terror. 'Things,' as Whewell truly says, 'would not lie where we placed them, but would slide away with the slightest push. We should have a difficulty in standing or walking something like what we have on ship-board when the deck is inclined; and we should stagger helplessly through an atmosphere thinner than that which oppresses the respiration of the traveller on the tops of the highest mountains.'

'It is hard,' says the ingenious writer in the 'Spectator,' 'to conceive even a one-storied house holding together,' in the Martian moon world, 'against any serious lateral blows.' Projectiles would be no less deadly than in our own world. But the range which projectiles such as ours would obtain in such a world would render close fighting impossible on the one hand and efficient aiming impossible on the other. A Krupp cannon, for instance, whose greatest range on our earth is, let us say, five miles, would on the Martian moon fire a projectile which would leave that moon for ever, and the recoil of the cannon would probably carry it half a dozen miles away from the firing point. Much weaker projectile force would have to be employed, and less mischief would be done on this account, and also because any living body struck by one of these projectiles would give way before it much more readily than a similar body similarly struck on our own earth.

The celestial appearances presented from the surface of Dread and Terror would be well worth describing did time and space permit, especially as in a sense they may be regarded as realities compared with the relations we have been dealing with. It is certain, for example, that to eyes such as ours Mars would appear from the nearest of his moons as a disc 1,400 times as large as our moon appears to us. (It is rather strange, by the way, that this is the exact size of the disc which Jupiter would present as seen from his nearest moon; Saturn, as seen from his nearest moon, appears only 900 times larger than our moon does to us.) But enough space has already been given to considerations which only have a speculative interest, and indeed must be regarded as of the most doubtful character even in this aspect. It has been well said by Sir J. Herschel that 'of such speculations there is no end;' and though it cannot be said that in such speculations there is no use—for they serve to remind us how extremely limited is the range over which our own experience can guide us—yet we must be content to consider these possibilities somewhat lightly. Their consideration must be regarded as scientific rambling, not as scientific study.

A RING OF WORLDS.

Three hundred years ago, when what was called the Copernican Paradox was struggling for existence against the then orthodox Ptolemaic astronomy, the solar system was supposed to consist of eight bodies. The followers of Copernicus believed in a central sun, round which six orbs revolved, while around one of these—our earth—travelled one other orb,—making (with the central sun) eight bodies in all. The followers of the old astronomy, including at that time nine-tenths of the astronomers of repute, believed in a central earth, round which travelled seven planets, the sun and moon being two of these, only distinguished from the rest (as planets) by the comparative simplicity of their movements. The number of bodies forming the solar system, without including comets or meteorites, or the multitudinous satellites which compose the ring of Saturn, has been raised of late to 200—so that for every orb known in the days of Copernicus and his first followers, twenty-five are now recognised by astronomers. Year after year more are becoming known to us. In fact, planets are being discovered so fast, that, after an effort (by dividing the watch upon them among the leading observatories) to keep them well under survey, the task has come to be regarded as almost hopeless. One or two of the flock are already missing; and it seems not improbable that, before many years have passed, twenty or thirty planets will have to be described as missing, while endless controversies may possibly arise, respecting those newly discovered each year, on the delicate question whether a discovery or a re-discovery has been effected.

It is hardly necessary to say, perhaps, that we refer to that strange ring of small planets which travels between the paths of Mars, the miniature of our earth, and Jupiter, the giant of the solar system, as far surpassing our earth in size as it is surpassed by the sun. In the wide space between these two planets wander thousands of tiny planets. They form a zone of division not only between Mars and Jupiter, thus unlike each other, but between the family of small planets of which our earth is the principal member, and the family of large planets—Jupiter, Saturn, Uranus, and Neptune. It is a strange thought that for ages these bodies have been circling round the sun unknown to men, though so near to us, compared with the fixed stars, that from the nearest of these the whole ring, far within which, be it remembered, the earth travels, would appear as the merest point in space. Still stranger is the thought that, among the members of this system or ring of worlds utterly invisible to ordinary eyesight, there must be presented at times, if living creatures are there to see, some of the most remarkable celestial scenery visible from any part of the solar system. For the orbits of these bodies interlace in a strangely complex manner. At times, from one or other of the set, several of the rest must be seen at so short a distance as to appear larger and more conspicuous than Jupiter or Venus appears to ourselves, while occasionally an even nearer approach must be made. In fact, in this part of the solar system, and in this part alone, collisions between planets are possible catastrophes; though, fortunately, the motions of these bodies being always in the same direction, they cannot encounter each other full tilt, but can only come into collision by the swifter overtaking the slower. Even of this there is little risk, so small are those planets, and so enormous the ring of space in which they travel.

For many years the idea had been gaining ground that those astronomers who were using their telescopes in the search for small planets were wasting time which might be better employed. Of what use, many asked, can it be, now that we know these bodies may be counted by thousands, to search night after night for hours on the chance of discovering a few each year? But recently it has been seen that the small planets may give us very useful information. They have in fact already told us how much their giant neighbour Jupiter would weigh if he could be put in a scale against the earth,—or rather (for that was already known) they have shown us that Jupiter had been rightly weighed in another way. And now it seems likely that we shall learn from this despised family the true measure of the sun's distance, and with that the scale of the solar system, the quantity of matter contained by the sun, and many other matters of great importance in astronomy.

As one of the longest known among the minor planets has already given a very fair answer to the questions of astronomers on such points, while two others have recently been put under examination, the occasion seems a suitable one for giving a brief account of this ring of worlds, of the manner of their discovery, and of the ideas which have been suggested as to their origin.

If the solar system could be seen at a single view, its appearance at any moment would give no idea of regularity in its construction. The pictures of the solar system in our books present a certain symmetry even when the paths of the planets are shown with their true eccentricity of position (which is, unfortunately, but seldom done). The symmetry is like that of a leaf or flower, not perfect, not geometrical or rigid, but still it is sufficiently striking. But if from a picture of the orbits, presenting this symmetry of appearance, we prick off the positions of the central sun and of the planets in various parts of their paths around him, we can see no symmetry at all in the resulting set of points. The solar system thus shows how there may be real symmetry of arrangement among bodies apparently scattered without law or order. And it shows us also the part which time plays in educing symmetry from apparent disorder. Conceive a being so constituted that the circuit even of the planet Neptune around the sun, though lasting more than a hundred and sixty of our years, would seem to last but a single instant, so that to his vision the planet would be visible during its entire circuit even as a spark swiftly whirled round appears as a circle of light. To such a being the solar system would present a symmetrical and doubtless a most beautiful appearance. At its centre would be the glowing orb of the sun, round which would appear four rings of light, representing the paths of Mercury, Venus, the Earth, and Mars; far outside these again four other rings of light, much brighter and with much wider spaces between them, showing where Jupiter and Saturn, Uranus and Neptune, traverse their wide courses; and between these families would be seen the multitudinous intertwining paths of the small planets, scarce discernible separately, but forming as a whole a faintly luminous ring between the well-defined sets of bright rings marking the paths of the eight planets. We need not here consider how the beauty of this scene would be enhanced by the rings of light which the moons of the giant planets and of our earth would produce. Let it suffice to note that the symmetry of the solar system, as thus seen, would be altogether marred if the ring of asteroids were removed. It is not given to man, whose span of life is less than half the orbital period of the outermost planet, to witness, scarcely even to conceive rightly, the scene we have described. But the mathematician can perceive what is necessary to its completeness. Accordingly, the astronomer Kepler, enquiring into the harmonies of the solar system, perceived that one note was wanting; or, returning to our ideal description of the system as it would be seen if centuries were fractions only of seconds, he perceived that the absence of a certain feature impaired the symmetry of the picture. He saw that though the distance separating the path of Mars from that of Jupiter is in reality much less than that which separates the path of Jupiter from that of Saturn, the next planet beyond him, yet there is a certain regularity in the progression of the distances which requires that the space between Mars and Jupiter should not be untenanted, as, according to the astronomy of his day, it was supposed to be. In his youth Kepler had noted the want, and had suggested certain fanciful relations which might be fulfilled by a planet occupying the gap. He had written to Galileo on the subject, who had advised him to base his theories on observed facts only. Later, when unwearying researches for nineteen years had revealed to him the laws of the solar system, Kepler suggested as the relation which connects the distances of the planets, that which is now commonly called Bode's law. It may be thus simply expressed: calling Mercury's distance from the sun 4, the distances of the other planets' orbits from Mercury's orbit are in order as the number 3, 6, 12, and so on, doubling as we proceed. According to this law the distance of Mars from Mercury's orbit should be 12, and the distance of the next planet 24. But there was no known planet at that distance. Jupiter, the planet next beyond Mars, travels at a distance from Mercury's orbit represented on this scale by 48, and Saturn—the most distant known planet—at a distance of 91, the former corresponding exactly, the latter fairly enough, with the law we have indicated. But the planet which, according to the law, should have travelled between Mars and Jupiter at a distance of 24 from Mercury's orbit, or 28 from the sun, either did not exist, or was invisible.

In Kepler's day it was thought by many a sufficient solution of the difficulty to conclude that a planet formerly travelling along this seemingly vacant track had been destroyed on account of the wickedness of its inhabitants. And we are told that there were not wanting preachers who used the destruction of this hypothetical planet as a warning to evil doers. If they continued in their sins they might not only bring destruction on themselves, but on the world, which might burst, as had that other world, and reduce the sun's family by yet another planet.[1]

It was not until the discovery of Uranus by Sir W. Herschel in 1781 that the speculations of Kepler attracted scientific attention. Astronomers had seen

[1] We do not learn whether the warning was effective or not; but probably the evil doers were not more troubled by a danger affecting the whole of the human race than by that which had long been described as hanging over themselves in particular. The logical effect of the warning, one would suppose, must have been to encourage that particular form of godliness which is shown by anxiety about the sins of others. For it was clearly very much to the interest of those who did well to see that the evil doers did not bring about a catastrophe from which good and bad alike could not fail to suffer.

the three laws of Kepler interpreted physically by Newton, and had come to regard those relations which admitted of no such interpretation as mere coincidences. But when the empirical law of distances, for which, as it appeared, no reason in nature could be assigned, was found to be fulfilled by the new planet, astronomers could not but regard the circumstance as somewhat more than a mere coincidence. It is strange to consider that had Neptune instead of Uranus been discovered by Sir W. Herschel, the very reverse would have been inferred. Mercury's orbit by Bode's law should be 96, but is really 91; that of Saturn's distance from Uranus should be 192, but is really 188, so that Bode's law is satisfactorily fulfilled by Uranus; but Neptune's distance from Mercury's orbit should be 384 and is really but 296, which cannot in any way be reconciled with the law. Supposing Uranus unknown when Neptune was discovered, the distance of Neptune would have seemed too great by 104 for Saturn's next neighbour (being 296 instead of 192), and too little by 88 for Saturn's next neighbour but one, according to Bode's law of distances. Thus astronomers would have inferred that Bode's law was erroneous (as indeed it is), and would not have thought of looking for a planet between Mars and Jupiter. As, however, by good fortune Uranus was found first, they inferred (mistakenly) that Bode's law represents a real relation existing, no one could say why, among the planetary orbits, and thence concluded (rightly) that the space between Mars and Jupiter is not vacant.

A society was therefore formed—chiefly through the active exertions of De Zach of Gotha—to search for the missing planet. It consisted of twenty-four astronomers under the presidency of Schroeter. The zodiac, the highway of the planets, was divided into twenty-four zones, one of which was assigned to each member of this Society for the Detection of a Missing World. The twenty-four commenced their labours with great zeal. When we consider that over the region of the heavens which they were to examine at least a hundred planets, well within the range of their telescopes, were travelling, we may fairly wonder that they discovered nothing. Such, however, was the result of their labours. After they had been at work a considerable time, accident revealed to an astronomer outside their society a body which was regarded for a long time as the missing planet.

Professor Piazzi, while observing stars for his catalogue, was led to examine very carefully a part of the constellation Taurus, where Wollaston had marked in a star which Piazzi could not find. On the first day of the present century he observed in this part of the heavens a small star, which he suspected of variability, seeing that it appeared where before no star of equal brightness had been mapped. On January 3 he found that the star had disappeared from that place, but another, much like it, lay at a short distance to the west of the place which it had occupied. The actual distance between the two positions was nearly a third of the moon's apparent diameter. On January 24 (our observer was not too impatient, it will be seen) he transmitted to Oriani and Bode, members of the Missing World Detection Society, an account of the movements of this star, which had travelled towards the west till January 11 or 12, and had then begun to advance. He continued his labours till February 11, when he was seized with serious illness. Unfortunately, his letters to Oriani and Bode did not reach those astronomers until nearly the end of March, by which time the planet (for such it was) had become invisible, owing to the approach of the sun to the part of the heavens along which the planet was travelling.

But the planet was not lost. The sun passed on his way through the region occupied by the planet, and in September that region was again visible at night. In the meantime, the great mathematician Gauss had calculated from Piazzi's observations the real path of the planet. Throughout September, October, November, and December search was made for the missing star. At length, on the last day of the year 1801, De Zach detected the planet, Olbers independently effecting the re-discovery on January 1, 1802. Thus the first night of the present century was distinguished by the discovery of a new planet, and before the first year of the century had passed the planet was fairly secured.

Piazzi, the discoverer of the planet, assigned to it the name of the titular goddess of Sicily, where the discovery was made—Ceres.

Ceres was found to be travelling in an orbit corresponding in the most satisfactory manner with Bode's law. According to that law the missing planet's distance from the orbit of Mercury should have been 24; calling Mercury's distance from the sun 4, the actual distance of Ceres is $23\frac{1}{3}$.

Yet astronomers were not satisfied with the new planet. It travelled at the right mean distance from the sun; but passing over its inferiority to its neighbours, Mars and Jupiter, in size and splendour, it moved in most unplanetary fashion. Instead of travelling nearly in the same plane as the earth, like its neighbours Mars and Jupiter, its path was inclined to that plane in an angle of more than ten degrees—a thing as yet unheard of among planets. As to its size, Sir W. Herschel, from measurements made with his powerful telescopes, estimated the new planet's diameter at about 160 miles, so that, supposing it of the same density as our earth, its mass is less than one 125,000th part of hers. Thus it would take more than 1,560 such planets to make a globe as massive as our moon. And even this probably falls far short of the truth. For our earth owes no small part of her density to the compression produced by the attractive energy of her own substance. The moon, which is less compressed, has much smaller density; in fact, little more than half the earth's. Mars, again, being smaller, and having less attractive energy, has less density than the earth (his density is about seven-tenths of hers).[1] The tiny Ceres would be very much less compressed, and, if made of the same

[1] Of course the giant planets Jupiter, Saturn, Uranus, and Neptune seem to present exceptions to the rule we have here indicated. But there can be no doubt that in their case intense heat expands the planets' substance, while in reality we have no

substances, as we may well believe, would probably have a density less than half the moon's, or not very much exceeding that of water. Thus, it would probably take some half million of worlds like Ceres to make such a globe as our earth, while from our moon six thousand such worlds as Ceres might be made. It was natural that astronomers should regard with some suspicion a planet falling so far short of every known planet, and even of a mere moon in size and mass.

But presently a discovery was made which still more markedly separated Ceres from the rest of the planetary family. Olbers, during his search for Ceres, had had occasion to study very closely the arrangement of the groups of small stars scattered along the track which Ceres might be expected to follow. What reason he had for continuing his examination of these groups after Ceres was found does not appear. Possibly he may have had some hope of what actually occurred. Certain it is that in March 1802, or nearly three months after Ceres had been re-discovered, he was examining a part of the constellation Virgo, close by the spot where he had found Ceres on January 1 in the same year. While thus at work he noticed a small star forming with two others known by him an equilateral triangle. He felt sure this star had not been there three months before, and his first idea was that it was a variable star. At the end of two hours, however, he perceived that it had

means of forming an opinion respecting their real density, since the surfaces we measure are not the real surfaces, but layers of clouds enwrapping these planets, and lying who shall say how far from the solid surface.

moved slightly towards the north-west. On the next evening it had moved still farther towards the north-west. It was in fact a planet, and, to the amazement of astronomers, the study of this planet's motion showed that its mean distance from the sun differed very little from that of Ceres. We speak of the amazement of astronomers, because the fact thus discovered was in reality the most surprising of any which had been made known to them since the nature of Saturn's ring was discovered by Huyghens in 1656. We have become so accustomed of late to the discovery of planets travelling along the region of space between the paths of Mars and Jupiter, that we are apt to forget how strange the circumstance must have appeared to astronomers at the beginning of the present century, that the old views respecting the solar system were erroneous, and that in addition to the planets travelling singly around the sun the existence of a ring of planets must be admitted. It is true that the discovery of this second planet (to which the name Pallas was given) did not fully demonstrate this. Still it showed that Ceres was not travelling alone in the region which had so long been supposed untenanted. And as it seemed in some degree to explain the smallness of Ceres, suggesting the idea that possibly the combined mass of bodies travelling in this space might not be greatly inferior to the mass of a primary planet, the notion of a ring of worlds travelling between Mars and Jupiter was presently entertained as according fairly with the facts already discovered.

Olbers himself was fully satisfied that other planets travel in the region between Mars and Jupiter. He was struck by the remarkable features of the orbit of the planet he had discovered. It was inclined more than three times as much as that of Ceres to the plane in which the earth travels, or to that medial plane near which lie the tracts of all the single planets. So greatly is the path of Pallas inclined to this track that even as seen from the sun its range on either side gave to the planetary highway a width of sixty-nine degrees, or nearly four times the width of the zodiac (the conventional highway assigned by the ancients to the planets) as determined by the range of Venus, viewed from the earth, on either side of the medial track. The range of Pallas as seen from the earth is still greater; so great, indeed, that this planet may actually be seen at times among the Polar constellations. Moreover, the path of Pallas is remarkably eccentric, insomuch that her greatest distance from the sun exceeds her least in the proportion of about 5 to 3. Olbers was led by these peculiarities to the belief that Ceres and Pallas are the fragments of a planet which formerly travelled between the paths of Mars and Jupiter, but had been shattered to pieces by a tremendous explosion. If our earth, as she travels along her present path, could by some violent internal action be shattered into fragments, the greater number of these would no longer travel in the plane in which lies the earth's present path. Those which chanced to be driven outwards in that plane would continue to travel in it, though on a changed path; for their original motion and their imparted motion both lying in that plane, so also of necessity would that motion which would result from the combination of these. But fragments which were driven away at an angle to that plane would not travel in it. Hence the great inclination of the path of Ceres and the monstrous inclination of the path of Pallas might be explained by supposing that the former was a fragment which had been driven away at a considerable angle to the ecliptic, while Pallas was a fragment driven away on a path nearly square to that plane.

To show more clearly how Olbers accounted for the peculiar motions of the new planets, suppose our earth to explode on or about March 20, at noon Greenwich time. Then the greater part of South America would be driven forwards; it would therefore travel on a course not far from the original track of the earth, but more quickly; our Indian Empire would be driven backwards; and though the advancing motion previously possessed by this part of the earth, in common with the rest, would still carry it forwards, this motion would be greatly reduced. The central parts of Africa and the Atlantic around Ascension Island and St. Helena would be driven sunwards—an impulse which, combined with the previous advancing motion of this region, would cause this part of their new track to cross their former nearly circular track at a sharp angle, passing athwart that track inwards. The part opposite to the last-named—that is, in the middle of the Pacific—would be driven directly from the sun, and this impulse, combined with advance, would cause this part of the new track of the scattered fragments from the Pacific to cross the original track at a sharp angle, passing outwards. All these regions, and all lying on the zone passing through them, would continue to move in or near the former plane of the earth's motion; some more quickly than before, some more slowly, some passing outwards at that portion of their course to return eventually inwards till they came to it again, and some passing inwards for awhile, to return, however, after a complete circuit, to the scene of the catastrophe. But England and other European countries would be impelled partly sunwards, partly upwards and northwards, from the plane of their former motion, and would therefore travel on a track largely inclined to their former course; that is, to the earth's present track. The same would happen, so far as upward motion was concerned, to the United States and to all the northern parts of Asia. The fragments from all these regions would thenceforward travel on inclined paths crossing their original track ascendingly at the place where the explosion occurred. On the other hand, Australia and New Zealand, South Africa, and the southern parts of South America, would be driven somewhat downwards or southwards, and the fragments of this zone of the earth would accordingly travel on paths crossing the original track of the earth descendingly at the place of the explosion. The North Polar regions, especially the parts north of the American continent, would be driven more directly upwards by the explosion; while the South Polar regions, especially the parts south of the Indian Ocean, would be driven as directly downwards: the fragments from these regions then would travel on paths most largely inclined to the original track of the earth.

Regarding the two planets hitherto discovered as fragments of one which had burst, Olbers perceived that there was a certain region of the heavens where he would have a better chance of discovering other fragments than anywhere else. Every fragment after the explosion would have a path passing through the place where the explosion occurred. For the place of explosion, being the spot from which each fragment started, would of necessity be a point along each fragment's future track. The fragments, be it understood, would not return simultaneously to that spot. Those which had been driven forwards (more or less), would have their period of circulation lengthened, those which had been driven backwards would have their period shortened; these last then would return to the scene of the outburst sooner than the former, and in point of fact no two would return simultaneously to that place unless, by some utterly improbable chance, they had been hastened or retarded in exactly the same degree. But all would pass through that spot for many centuries after the terrible catastrophe which had scattered them on their various paths. If the region of the heavens towards which that spot lay could be determined, then, the careful observation of that region probably would soon be rewarded by the discovery of other fragments. Moreover, the region exactly opposite to it would be similarly suitable for the search after these small bodies; for though their paths would not all pass through a *point* exactly opposite the scene of the explosion, these paths would all pass through the prolongation of a line drawn through the sun from that place. This is easily seen. Every planet has its own plane of motion, in which plane the sun necessarily lies; if, then, we know any one point of a planet's path, we know that the line joining the sun and that point lies in the plane of the planet's motion, and if extended beyond the sun must cross the planet's track.

Olbers then set himself the task of carefully observing two parts of the heavens, one being the place where the tracks of Ceres and Pallas approached each other nearest, the other being the place directly opposite to this. One point is to be noticed as essential to Olbers's faith in the success of his method of search. In his day it was generally believed that many centuries had not passed since the planets had been set moving on their respective paths. According to this view the catastrophe by which Ceres and Pallas and the fragments yet to be discovered had been sent on their new courses, could not have occurred so long ago that the paths of the fragments had been materially displaced from their original position. If, on the other hand, millions of years might have elapsed since the catastrophe happened, there would have been little room for hoping that the actual paths of the fragments would have retained any trace of the peculiarity we have described. It was somewhat fortunate for science that Olbers had full faith in the doctrine that the date of the catastrophe could not be more than four or five thousand years before his time, and that therefore he observed the two regions of the heavens indicated by the explosion theory with unwearying assiduity for many months. He also persuaded Harding, of Lilienthal, to pay special attention to these two regions; one near the northern wing of the Virgin, the other in the constellation of the Whale.

At length, on September 4, 1804, the search was rewarded with success; the planet called Juno being discovered by Harding in that part of the Whale which Olbers had indicated. Olbers did not cease from the search, however, but continued it for thirty months after Harding's success, and five years after his own discovery of Pallas. At length on March 28, the fifth anniversary of this discovery, Olbers detected Vesta, the only member of the family of asteroids which has ever (we believe) been seen with the naked eye.

For some reason astronomers seem to have been satisfied with this fourth fragment of Olbers's hypothetical planet. The search was not resumed for twenty-three years. Then Hencke, an amateur astronomer of Driessen, in Germany, commenced a search destined to meet with no success until more than fifteen years had elapsed. We shall return presently to the discovery of the fifth asteroid by Hencke. We must, first, however, consider the interesting questions raised by astronomers, after the discovery of Vesta, upon the theory of Olbers that the asteroids are fragments of an exploded planet.

Lagrange, in 1814, examined the theory mathematically, enquiring what degree of explosive force would be necessary to detach a fragment of a planet in such sort that it would not return, but travel thereafter on an orbit of its own around the sun. We have not by us the result of his researches except as they are given in Grant's Physical Astronomy, as follows: 'Applying his results to the earth, Lagrange found that if the velocity exceeded that of a cannon-ball in the proportion of 121 to 1, the fragment would become a comet with a direct motion; but if the velocity rose in the proportion of 156 to 1, the motion of the comet would be retrograde. If the velocity were less than in either of these cases, the fragment would revolve as a planet in an elliptic orbit.' This statement is not very satisfactory, because the velocity of a cannon-ball, depending considerably on circumstances, is not a definite unit of measurement. The assertion, too, that the fragment would become a comet is open to exception, and nothing is said about the least velocity necessary to free the expelled body from the earth. Probably the velocity of a cannon-ball was taken by Lagrange at about 500 yards per second, that being a fair velocity for a 68-pounder at the date of his paper. A velocity, then, exceeding a cannon-ball in the proportion of 156 to 1, would be about 44 miles a second. Now, for a body expelled from the earth to travel as a retrograde comet, it must be sent backwards with a velocity equal to the earth's in her orbit (about $18\frac{1}{2}$ miles per second), increased by the proper velocity for a retrograde comet, about $25\frac{1}{2}$ miles per second, or 44 miles per second in all. This agrees, then, with Lagrange's result. But he seems to have been led from the real subject of enquiry to problems which are only matters of curiosity. The fragmentary planets of Olbers's theory move neither as advancing nor as retrograde comets. Leaving, then, Lagrange's paper, as not very much to the point, if rightly represented by Grant, we note simply that the velocity necessary to expel from the earth a fragment of her mass in such sort that it would not be drawn back, would amount to about seven miles per second, or, say, about twenty-five times the velocity of a cannon-ball.

But, again, the expulsion of a fragment and the explosion of an entire planet are processes very different in their nature. If a fragment were expelled, the entire mass of the earth would recoil with a motion bearing the same kind of relation to that of the fragment which the recoil of a very heavy cannon bears to the motion of the ball. If a cannon were not heavier than the ball, the cannon would be driven back as rapidly as the ball would be expelled, though frictional resistance would bring it sooner to rest. Again, when a shell at rest bursts, the fragments are driven outwards on all sides, with much smaller velocities than any one of them would have if the entire charge of powder acted upon it, the rest of the shell being in some way restrained from moving. We see, then, that for a planet to explode into fragments which thereafter should be free to travel independently around the sun, the explosive force must enormously exceed what would be necessary in the case of a single fragment expelled as a projectile is expelled from a gun.

When we consider, further, that the frame of the earth is demonstrably not the hollow shell formerly imagined, but even denser at its core than near its surface; that, moreover, it is not formed of rigid materials, but of materials which under the forces to which they are subject are perfectly plastic and ductile, it seems incredible that under any conditions which appear possible our earth could be shattered by an explosion. Professor Newcomb, of Washington, in an able paper on this subject, remarks on this objection that, 'since the limits of our knowledge are not necessarily the limit of possibility, the objection is not fatal, and it is difficult to say what weight ought to be attached to it;' and, as many of our readers will remember, Sir W. Thomson, one of the greatest mathematicians living, has not thought the arguments against the possible or probable shattering of a planet sufficiently weighty to prevent the theory from being entertained that one world may be peopled from the seeds of life brought to it by the fragments of another which had exploded. Yet it may fairly be said that if the destructive explosion of a planet is possible it is utterly improbable; and that absolutely nothing is at present known to us which suggests even the bare possibility of such a catastrophe.

Yet the theory that a planet which had been travelling between Mars and Jupiter had burst into fragments had a much more probable appearance in Olbers's time than it has at present; for the four asteroids first discovered travelled on orbits not differing greatly as to their mean distances, which are as the numbers 236 (Vesta), 267 (Juno), 277 (Ceres and Pallas). When asteroids began to be discovered which travelled nearer to the sun than Vesta, and much farther away than Ceres and Pallas, the explosion theory was shown to be improbable. When, further, the actual paths of these multitudinous worlds came to be examined, the theory was found to be utterly untenable. More recently still a circumstance noted by the ingenious American astronomer, Kirkwood, has pointed to another theory as extremely probable.

The history of the successive discovery of the various members of the asteroidal family, though not without interest, would be little suited to these pages. A few details, however, may be mentioned here as illustrating the general character of the search.

We have seen that Hencke engaged in 1830 in the search for a fifth asteroid. On the evening of December 8 he observed a star of the ninth magnitude in the constellation Taurus, in a place where he felt sure, from his recollection of the region, that there had previously been no star of that degree of brightness. He communicated the observation to Encke, of Berlin; and on

December 14 they rediscovered it in the place to which by that time it had removed. It was found to be an asteroid travelling at a distance almost midway between that of Vesta and that of Ceres. Hencke requested Encke to name the new planet, and that astronomer selected for it the name of Astræa.

On July 1, Hencke discovered a sixth asteroid, which Gauss named at his request, calling it Hebe. In the same year, and only six weeks later, our English astronomer Hind discovered the asteroid Iris; and on October 18 he discovered another, to which Sir J. Herschel, at his request, assigned a name, selecting (somewhat unsuitably, perhaps, for an October discovery) the name Flora.

Since that date, not a year has passed without the discovery of at least one asteroid, as in 1848, 1849, and 1859. Two were discovered in 1851, 1863, and 1869; three in 1850, 1864, 1865, and 1870; four in 1853, 1855, and 1867; five in 1856, 1860, 1862, and 1871; six in 1854, 1858, 1866, 1873, and 1874; eight in 1852 and 1857; ten in 1861; eleven in 1872; twelve in 1868 and 1876; and seventeen in 1875. The astronomer who has hitherto been most successful in the search for asteroids is Peters, of Clinton, U.S. (Professor Peters is a German by birth, however), with twenty-seven; next Luther, of Bilk, with twenty; and third Watson, of Ann Arbor, Michigan, with twenty. Goldschmidt, a French painter, discovered fourteen; Borelly and our Hind, ten. These six have thus discovered 101 of the asteroids at present known. After them come De Gasparis and Palisa, with nine each; Pogson, of Madras, with seven; Chacornac and Paul Henry, with six each; Prosper Henry and Tempel, with five, and Perrotin, with 4. Of the remainder three were discovered by Ferguson; two by Olbers, Hencke, and Tuttle; and Piazzi, Harding, Graham, Marth, Laurent, Searle, Forster, d'Arrest, Tietjen, Stéphan, Coggia, Schulhof, Schiaparelli, and Knorre have each discovered one.

Some coincidences which would seem curious but for the great number of asteroids already known have naturally occurred during the progress of discovery. Thus the asteroid Irene was discovered by De Gasparis, independently, a few days after Hind had marked it for his own (May 19, 1851). *En revanche*, De Gasparis discovered Psyche on March 19, 1852, while Hind, who had seen the planet on January 18, but had been prevented by bad weather from re-observing it, satisfied himself on March 18 of its planetary character. While Hind was planning a vigorous search after the planet, news reached him that De Gasparis had discovered it. Goldschmidt, on September 19, 1857, discovered two asteroids, which chanced that night to be within a distance from each other equal to about one-third of the apparent diameter of the moon. No other astronomer has ever had the good fortune to capture two of these wandering bodies on the same night and within the same telescopic field of view. But the planet Alexandra was discovered by Goldschmidt, at Paris, on September 10, 1858, and the planet Pandora by Mr. Searle, of Albany, New York, on the same night, only a few hours later. The asteroid Melete, really discovered on September 9, 1857, was not recognised as a new planet till 1858, having been for a long time mistaken for the asteroid Daphne. The latter had been lost since May 1856, and Goldschmidt, its discoverer, was looking for it in September 1857, when he found Melete. When Melete was proved by Schubert's calculations to be a different body, fresh search had to be made for Daphne; but she was not found till August 31, 1862, having been thus lost more than six years.

One feature of M. Goldschmidt's labours in this field of research is worthy of mention. Most of the astronomers who have added to the list of known asteroids were professional observers, employed in well provided observatories. Goldschmidt was a painter by profession, and the telescopes with which he observed were successively, as he could afford to extend his observational resources, of 2 inches, $2\frac{2}{3}$ inches, and four inches' aperture only. 'None of M. Goldschmidt's telescopes,' says Mr. Main, of the Radcliffe Observatory, 'were mounted equatorially' (that is, so as to follow any star to which they might be directed by a single motion), 'but in the greater number of instances were pointed out of a window which did not command the whole of the sky.'

Having now nearly two hundred of these bodies to deal with, we can form a safer opinion, than in Olbers's time, of the theory whether they are fragments of an exploded planet. The answer to this question comes in no doubtful terms. One fact alone suffices to show clearly that they cannot have had a common origin. The least distances of some of the more remote of these bodies from the sun exceed the greatest distances of some of the nearer. Thus Harmonia, at her greatest distance from the sun, is about 217 millions of miles from him, Nemansa 231 millions, Feronia 233 millions, and so on; while Cybele, at her nearest, is 276 millions of miles from the sun, Doris 262 millions, Hygeia 259 millions, and so on. So that Cybele, at her nearest to the sun, is farther from him by nearly 80 million miles than Harmonia at her nearest. The two orbits do not even approach each other within this distance, enormous though it is, for the place of Cybele's nearest approach is not *nearly* in the same direction from the sun as the place of Harmonia's greatest recession. The two orbits nowhere approach within a distance less than that which separates our earth from the sun. If the two planets were originally parts of a single one, their orbits after the explosion would have intersected. It is utterly impossible that, if this had been so, subsequent perturbations could have separated the paths by so enormous a distance as 90 millions of miles at the place even of nearest approach.

But while the discovery of multitudinous members of this ring of worlds has rendered Olbers's theory of the explosion of a single planet between Mars and Jupiter utterly untenable, it has brought to our knowledge a remarkable relation which points very clearly to the real origin of the ring system of planets.

When as yet only half as many asteroids had been discovered as are now known, Professor Kirkwood, of Bloomington, Indiana, arranging these bodies in the order of their mean distances from the sun, noticed that certain gaps exist, in such sort that no asteroids travel at or nearly at certain mean distances from the sun. And looking more closely into these missing distances, he observed that they correspond to the distance of the giant planet Jupiter in this way, that a planet travelling at any one of these missing distances would have motions synchronising with those of Jupiter, in the same sense in which the vibrations of one note synchronise with the vibrations of another in harmony with it. For instance there is a well-marked gap at a distance from the sun exceeding our earth's in the proportion of 5 to 2; now a planet travelling at this distance would make three circuits while Jupiter makes one. There is another gap at a distance somewhat exceeding three and a quarter times the earth's; and a planet at this distance would travel twice round the sun while Jupiter travels once round him. Still more remarkable, because occurring in the very heart of the ring, is the gap corresponding to the distance of a planet which would travel five times round the sun while Jupiter travels twice round him. There are two gaps, also, where a planet would travel seven times round (1) during two circuits, and (2) during three circuits, of Jupiter.

Before enquiring into the meaning of this peculiarity, we note that now, when twice as many asteroids have been discovered,[1] the peculiarity is better marked even than when Kirkwood first noticed it. He was justified in saying, as he did in 1868, that the coincidences are not accidental; for the odds were enormously against the observed arrangement, and its accidental occurrence so unlikely as to be practically impossible. But had the arrangement been accidental with the eighty-seven asteroids known to Kirkwood, it could not but have happened that some of the eighty-nine since discovered would have had mean distances corresponding to those gaps or *lacunæ*. This, however, has not only not happened, but the aggregation of asteriods at distances where Kirkwood had already noticed that they were most numerous, has become still more decided.

We are led back, in our enquiry into the significance of this singular relation, to the time when our solar system was gradually forming from its former nebulous condition. Imagine a ring of nebulous fragments, not as yet gathered into a single mass. The process of aggregation would depend in considerable degree on the disturbances to which the fragments were exposed. If they were all moving in concentric orbits, and were not disturbed at all, there would be no collisions, and they would remain as a ring of fragments. It might seem, then, at a first view, that the

[1] This was written in 1878. It has not been thought well to alter the wording of the article as the evidence has been corrected only to the year 1878.

zone of asteroids was most favourably placed for aggregation into planet form, being under the special perturbing influence of Jupiter, the mightiest of all the planets. But excessive disturbance would be by no means favourable to the formation of a single planet. The nebulous matter must be churned by perturbations, but it must not be scattered by them; and this is what Jupiter's action on the planetoidal ring has done. Quantity of matter, again, would be a very important point in the process of aggregation. A region crowded with nebulous fragments would soon teem with aggregations, which would before long gather into a few large masses, which in turn would aggregate into one. But in a region where nebulous matter was very sparsely strewn, aggregations would not readily form, however mightily the region might be disturbed. The very activity of the disturbing forces might, in this case, check the process of aggregation. The two bodies which had once come into collision would travel on intersecting orbits, and would therefore before long come into collision, if not perturbed; but if perturbed, their orbits would cease to coalesce; so that the action of a great disturbing planet might prevent a process of aggregation which had already commenced. Now, we know that the quantity of matter in the region where the asteroids travel is less than in any other zone of the solar system. We do not know how many asteroids there are, but we do know how much they all weigh; at least, we know that altogether their weight is not more than a fourth of our earth's, and is probably a great deal less. And the zone over which they range is very much larger than the zone over which our earth may be regarded as bearing sway. Their zone being thus poverty stricken, and Jupiter's mighty mass in their neighbourhood perturbing them too actively to allow of their aggregation, they remain as a ring of fragments.

And now let the signs of Jupiter's influence in this respect be noticed. He would perturb all these fragments pretty equally in a single revolution of his. But those whose periods synchronised with his own would be more seriously perturbed. For the disturbance produced in one set of revolutions which brought any asteroid and Jupiter back to the position they had before those revolutions began, would be renewed in the next similar set, and in the next, and so on, until one of two things happened. Either the asteroid would be thrown entirely out of that periodic motion which had brought it thus under Jupiter's effectively disturbing influence, *or*, being set travelling on a markedly eccentric path, it would be brought into collision with some of the neighbouring asteroids, and would cease to have separate existence, or at least move thenceforward on a changed orbit. Thus those asteriods having a period synchronising with that of Jupiter would be gradually eliminated, and we should find gaps in the ring of worlds precisely where gaps actually exist.

There can be no reasonable doubt that these marked gaps were produced in the manner here described. Their existence can indeed be explained in no other way, and can be so satisfactorily explained in this way that assurance is made doubly sure.

But now consider the significance of this result. Imagine the asteroidal ring as it now exists to be redistributed, the gaps being filled up. The process we have described would immediately come into operation. But many millions of years would be required before it could eliminate even a few among the asteroids having those synchronous periods which expose them to accumulating perturbations. Only one of the two processes above described would really be effective. Mere change of period would be oscillatory. We have an instance of the kind in the motions of Jupiter and Saturn, which very nearly synchronise, Saturn going almost exactly twice round the sun while Jupiter goes five times round. But though for a long period of time accumulating perturbations lengthen Saturn's period (and shorten Jupiter's), after a while the time comes when these changes are reversed; then Saturn's period begins to shorten (and Jupiter's to lengthen). The changes carry these periods on either side of their mean value, just as the swinging of a pendulum carries it on either side of its mean position. So it would be with an asteroid mightily perturbed by Jupiter; its period would oscillate more widely, but still it would oscillate; and during the middle of the oscillation (just as a pendulum at the middle of its swing is in its mean position) the asteroid would have that synchronous period which, as we have seen, none of the asteriods in point of fact possess. We must look, then, to collisions to cause the gaps in the ring of worlds. But how rare must such collisions be among minute bodies like the asteroids, even though they be hundreds of thousands in number, occupying a domain in space so vast as that which belongs to this system! The width of the ring greatly exceeds the earth's distance from the sun, amounting in fact to more than 120 millions of miles. Its innermost edge is more than 200 millions of miles from the sun. It is not a flat ring, but shaped like an anchor ring (or a wedding ring), and is as thick as it is wide—insomuch that a cross section of the ring would be a mighty circle, more than 120 millions of miles in diameter. Amidst this enormous space a million asteroids, each 500 miles in diameter (and none of the asteroids are so large, while the number even of those exceeding 100 miles in diameter scarce amounts to a hundred), would be as widely scattered as a million grains of sand would be in such a space as the interior of St. Peter's, at Rome. Take a cubical block of sandstone, one inch in length, breadth, and thickness, crumble it into finest sand-dust, and imagine this dust scattered in the interior of that great building. How small would be the chance that any two particles from that tiny heap would come into collision during months of their aërial wanderings! Very much smaller would be the risk of a single collision between asteroids during millions of years as they travel (all the same way round, be it noticed) on their wide orbits, even though their number were a hundredfold greater than it is, and their volumes increased a millionfold.

Either, then, we must imagine innumerable millions of years to have elapsed since the ring of asteroids first existed, and that very gradually the synchronous asteroids have been eliminated by collisions, or else we are forced to the conclusion that the formation of this ring of worlds, or rather this series of rings, belongs to an earlier era of our solar system's history, when the matter from whence the rings were one day to be formed was in the nebulous condition. It appears to us that the latter conclusion is altogether the more probable. We escape none of the difficulties of the problem by adopting the former conclusion, while many other difficulties are introduced. By the latter we simply have the same difficulties to encounter which appertain to all forms of the nebular hypothesis respecting the origin of the solar system. These difficulties are great, because the distance over which we endeavour to look back is great; but they are not insuperable. The positive evidence for the general theory becomes stronger and stronger as astronomical research advances; and the mere circumstance that it is surrounded by difficulties can in no sense lead us to abandon it, although compelling us to admit that as yet we have not thoroughly mastered its details. The asteroids themselves supply an argument in favour of the nebular theory rendering its probability so strong as practically to amount to certainty; for the antecedent probability against the observed uniformity of direction of the 175 asteroids by chance, or in any conceivable way except as the result of some process of evolution, is equal to that of tossing either 'head' or 'tail' 175 times running, or about

23,945,290,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000,000 to 1.

Adopting the nebular theory, we must of course adopt with it the conclusion respecting the origin of the asteroids, to which, as we have seen above, we are led by the examination of the relations presented by this system—viz. that while still existing as a great ring of nebulous masses, they were to such degree perturbed by Jupiter's mighty attraction upon them as on the one hand to be prevented from forming into a single planet, and on the other to be sorted out, if one may so speak, into several rings with well-marked gaps between them, these gaps corresponding *exactly* with the distances at which planets would be most effectively disturbed by Jupiter. The close accordance between the results to which we are led by *à posteriori* and *à priori* considerations, affords strong evidence in favour of both lines of reasoning. But it is very noteworthy, also, that when, seeing the probability of the conclusions towards which we have been led, we enquire whether any similar case exists within our solar system, and if so whether the evidence in that case corresponds with that which we have obtained in the case we have been considering, we find the most striking evidence of all. The ring system of Saturn has long been regarded as consisting of multitudes of minute satellites. Thus it resembles the zone of asteroids, only it is relatively much more crowded. Now, in the ring system of Saturn there are gaps or relatively vacant divisions separating rings of closely clustering satellites. Distinguished among all these gaps by superior breadth and darkness is the great division separating what were formerly called the two rings from each other. Here, for a breadth of nearly 2,000 miles, so few satellites travel that to ordinary observation the great division looks black, though, closely scrutinised, it is found to be simply very dark. Now when we enquire whether satellites moving round this open space would have periods synchronising with that of the innermost (and therefore most effectively disturbing) of his moons, we discover these remarkable facts—that a satellite would travel in the very middle of the dark division or open space if its period were one-half that of the innermost of Saturn's moons, and almost on the same track if its period were one-fourth that of the innermost moon but two, while it would be well within the open space, but nearer its inner edge, if its period were one-third that of the innermost moon but one, or one-sixth that of the innermost moon but three. It follows unmistakably from these relations, first noted by Professor Kirkwood, that the great division in Saturn's rings has been swept and garnished by the action of the four innermost of Saturn's moons, but especially by the innermost of all. This fact corresponds so well with the nebular hypothesis, and is so utterly inexplicable on any other, as strongly to corroborate an opinion, expressed by the present writer in 1866, that the peculiarities of the Saturnian ring system would one day be found to afford 'a key to the law of development under which the solar system has reached its present development.' The same may now confidently be said respecting the ring of worlds travelling between the orbits of Mars and Jupiter. It has already enabled us to weigh the giant Jupiter afresh; it has given excellent measures, and promises to give yet better measures, of the dimensions of the solar system; and we venture to predict that before long this zone of worlds will have placed beyond shadow of doubt or question the general theory of the development of our solar system of which Laplace's nebular hypothesis presents only a few details or rather suggests only a few possibilities.

EARTH-BORN METEORITES.

So many circumstances which had before seemed mysterious in the phenomena presented by meteors and falling stars have of late years been explained by what may be called the astronomical theory of their origin, that students of science have been apt to throw (perhaps somewhat too hastily) into the background, the theory of the terrestrial origin of some at least amongst these bodies. Indeed it may be remarked as a somewhat prevalent mistake in the discussion of scientific views, to recognise in the demonstrated justice of one theory the necessary failure of another purporting to explain the same phenomena. For instance, because it has been shown unmistakably that many faint stars are nearer to us than some of the brightest, the inference is adopted that the brighter stars are not the nearer; whereas, in reality, the choice does not lie between the two theories—that the brightness of a star indicates proximity, and that it indicates superior real size and splendour—seeing that it is possible, nay one may in this case say it is certain, that both views are partly true. On the whole doubtless the brighter stars are nearer than the fainter, and again on the whole there are relatively more of the largest orders of stars among those which appear bright than among those which appear faint. The case of meteors and shooting stars is not altogether so simple. Yet it is certain that the clear and satisfactory proof astronomers have obtained of the extra-terrestrial origin of a great number of meteors, does not involve, of necessity, the conclusion that none among the bodies which from time to time reach our earth from without, have had a terrestrial origin in remote past ages when the earth's condition was very unlike that which we now recognise. Recently some evidence of a rather striking kind has been obtained from the constitution of meteoric masses to show that such has indeed been the origin of some meteorites. An astronomer well able to discuss the mathematical relations involved has found reason to regard the theory with favour, if not absolutely accepting it. Some indeed fall into the same fault here into which (as we think) those had fallen who inferred as a necessary conclusion from other evidence that no meteorites can possibly be of terrestrial origin; for they seem disposed to regard all meteorites of a certain large and important class as originally earth-born. We propose now to consider briefly the nature of the evidence in favour of the terrestrial origin of some among the bodies which fall from time to time upon the earth, and then to enquire how far it is likely that the evidence applies to all the members of that particular class of bodies.

In the first place we must briefly indicate the position of meteoric astronomy at the present time.

It will be remembered by many of our readers that after the meteoric display of November 14, 1866, astronomers succeeded in rapidly bringing together a mass of evidence, cogent at first, but presently found to be overwhelming, in favour of the theory that shooting-stars are bodies travelling in orbits of vast extent around the sun. They were able to ascertain the precise figure and position of some among these orbits, owing to the recognition of the strange circumstance that the two best known systems of shooting-stars travel in the track of two comets, one large and conspicuous, the other telescopic. If they had been in any doubt as to the validity of the reasoning by which this conclusion had been established, all such doubts would have been removed by the observations made on the system of meteors following in the track of the comet called Biela's. It was predicted that on the night of November 27, 1872, when it was known that the earth would pass through the track of that comet, a shower of falling stars would be seen, radiating from a part of the heavens near the feet of the constellation Andromeda, or the Chained Lady—that being the direction from which bodies following in the train of Biela's comet would seem to traverse our skies if, as the earth travelled onwards, they overtook her, and were rendered luminous in their rush through our atmosphere. On that evening, a wonderful display of meteors was seen, thousands being counted by European observers, while according to one account the above-named regions of the heavens from whence, as predicted, the meteors radiated, was aglow with an amber-coloured light, as though illuminated by tens of thousands of faint meteors too minute to be individually discernible. Nor was this all. A European astronomer named Klinkerfues thought that it might be well to direct the attention of astronomers whose observatories commanded the southern heavens to the circumstance that a flight of meteors following in the train of Biela's comet had swept over the earth from the direction of the northern stars marking the feet of Andromeda, and that therefore possibly the flight might be seen (as a whole) travelling onwards towards the southern stars which lie exactly opposite those northern ones. Accordingly he telegraphed to Mr. Pogson, of the Madras Observatory, 'Biela touched earth on November 27; look for it near Theta Centauri.' Pogson examined that part of the heavens, and there discovered two faint cloudlike objects presenting the appearance of small comets. These, whatever they were, were not star-clouds or nebulæ, for they were seen to be in slow motion athwart the heavens; although it appeared, on further enquiry, that neither could have been the flight of meteors which had swept over the earth (or through which the earth had passed) on November 27. While it was certain that Biela's comet itself was at least twelve weeks' journey further on than these comets (assuming they were really travelling in its track), yet their motion corresponded with the theory that they belong to the train of cometic matter following after Biela's comet, to which beyond all doubt belonged also the flight of meteors which produced the display of falling stars on the night of November 27–28.

But here, in passing, we must correct a notion into which many persons little acquainted with astronomy have fallen, when they have learned that meteors of different orders follow, in flights of many hundreds of millions, in the track of known comets, imagining that the mystery of comets' tails can thus be readily explained. The track of a comet and the tail of a comet are not coincident. If they were, it would of course be natural enough to suppose that when we look at the long tail of such a comet as Newton's or Donati's, we see in reality the stream of meteoric attendants following after the head or nucleus of those splendid objects. This theory has indeed been elaborated by a mathematician of repute, who has fallen into the mistake of supposing that the tail of a comet coincides with the track which the head of the comet pursues in space. It is so easy even for a man of science to fall into a mistake of this kind in dealing with a matter outside the subject of his special study, that we should not be careful to notice the error were it not, first, that it may mislead many, and, secondly, that the mathematician in question, Prof. P. G. Tait, of Edinburgh, has rather a failing for dealing severely (not to say sourly) with errors of the kind, or even with far slighter errors made by others. We may thus at once correct a mistaken notion about a scientific subject, and at the same time we may perhaps teach a too censorious critic to understand how readily even the most careful (for such, considering his severity, we must suppose him to be) may fall into gross and palpable errors.

The first part of the following quotation is correct enough and well worth studying as a sound, if not very elegant, exposition of the visibility of flights of meteors. The fault is in the application at the end; we may say of the 'passage,' its sting is in its tail. 'Let us consider,' says Prof. Tait, 'a swarm of meteorites' (regarded each as a fragment of stone) like a shower, in fact, of macadamised stones, or bricks, or even boulders, —'what would be the appearances presented by such a cloud? It must in all cases be of enormous dimensions, because the earth takes two or three days and nights to pass through the breadth of the stratum of the November meteors. Consider the rate at which the earth moves in its orbit, and you can see over what an enormous extent of space these masses are scattered. Now, if you think for a moment what would be the aspect of such a shower of stones when illuminated by sunlight, you will see at once that, seen from a distance, it would be like a cloud of ordinary dust; and an easy mathematical investigation shows that it should give when sufficiently thick, except in extreme cases, a brightness equal to about half that of a solid slab of the same material similiarly illuminated. The spectrum of its reflected or scattered light should be the spectrum of sunlight, only a great deal weaker. It is easy without calculation, by simply looking at a cloud of dust on a chalky road in sunshine, to assure one's self of the property just mentioned of such a cloud of dust or small particles. Remember that in cosmical questions we can speak of masses like bricks, or even paving-stones' (!) 'as being mere dust of the solar system, and we may suppose them as far separated one from another, in proportion to their size, as the particles of ordinary dust are. Whether then it be common terrestrial dust, or cosmical dust, with particles of the size of brickbats or boulders, does not matter to the result of this calculation. Spread them about in a swarm or cloud as sparsely as you please, and only make that cloud deep enough and illuminate it by the sun, then it can send back one half as much light as if it had been one continuous slab of the material. Now, look at the moon. You see there a continuous slab of material, and you know what a great amount of brightness that gives. And a shower of stones in space at the same distance from the sun as the moon, and of the same material as the moon, could, if it were only deep enough, however scattered its materials, shine with half the moon's brightness. Now, no comet's tail has ever been seen with brightness at all comparable to that of the moon; and therefore it is perfectly possible, and, so far as our present means enable us to judge, it is extremely probable, that the tail of the comet is merely a shower of such stones.' . . . 'This excessively simple hypothesis,' he says further on, after considering how the conflict of meteoric bodies composing a flight might generate the light of the comet's head and coma, and account for the appearance of jets extending from the nucleus sunwards, and thence streaming backwards to form the tail, 'appears easily able to account for many even of the most perplexing of the observed phenomena. I must warn you, however,' he concludes, honestly and frankly enough (he is by no means always as just in attacking the mistakes of others as in defending his own), 'that this is not the hypothesis generally received by astronomers.'

The hypothesis is in fact utterly untenable, as every astronomer, or even everyone acquainted with the astronomical history of a single large comet, knows well. It may suffice to point out that the tail which we do see extends from the head in a direction exactly opposite from the sun's (the tail may be, and often is, curved markedly, at some distance from the head, but it invariably extends from the head, exactly in the direction mentioned); and this direction can never be the track of any comet except one travelling directly towards the sun. It need hardly be said that no comet has ever been seen to travel in that direction; if a comet ever should be seen to travel in that way, we shall have an opportunity of learning whether Newton was right in supposing that the downfall of a comet on the sun would cause an outburst of solar heat by which terrible mischief would be wrought upon our earth. But while we do see a well-defined stream of cloudy light in a direction which does not coincide with a comet's track, but is often largely inclined to it, and not unfrequently almost exactly opposite the track, we have never yet succeeded in tracing the faintest luminosity along any part of the track of a comet, even where we have reason to believe that meteoric attendants are most numerous. The only case in which a cloudy light has been recognised on a comet's track has already been referred to—the case of Biela's comet and the two cloud-like objects seen by Pogson. But even in this case, which does not in the remotest degree correspond with Professor Tait's idea, we have every reason to believe that actual though subsidiary comets were observed; for Biela's comet divided early in 1846 into two distinct comets, and as it has since been entirely lost, though astronomers were well acquainted with the course it should have pursued since, and have searched for it with excellent telescopes, we may reasonably believe that the comet is now broken up into fragments, two of which Pogson probably saw.

However, it has been fairly demonstrated that large numbers of meteors falling as shooting-stars are bodies which had been travelling in the tracks of comets before encountering our earth, and turning to vapour in their rush through its atmosphere. Now, the question had long since arisen how the flights of meteors thus travelling in orbits more or less elongated around the sun, had been caused to pursue their present paths. Schiaparelli, of Milan, advocated the theory that comets which on their voyage from interstellar space towards our solar system chanced to pass near one of the planets, especially if such a planet were one of the giant planets, would be diverted from their former course into an orbit necessarily passing through the spot where the comet's motion had been thus affected. In other words, the new orbit of the comet would intersect or pass very near the orbit of the disturbing planet. It is singular that the astronomers, including such able mathematicians as Sir George Airy and the late M. Leverrier, who accepted this explanation, should have overlooked the overwhelming objections which exist against it. In the first place, it is obvious that for every comet captured, so to speak, in this way, not millions, but millions of millions, would escape; and we should have to form a much more extended estimate of the total number of cometic systems in the universe than has been usual, or than can be regarded as admissible. But this is not the most serious objection to Schiaparelli's theory. So soon as we inquire how near a comet arriving from remote interstellar space must pass to Neptune, or to Uranus, or to Saturn or to Jupiter, in order to be compelled to travel in an orbit not extending far beyond the spot of nearest approach, we find so near an approach to be necessary that a comet of average size would have but a small proportion of its mass suitably deflected—the rest would pass too near, and be there and then drawn down to the surface of the disturbing planet, or would not pass near enough, and so would travel thereafter on an entirely different orbit from that followed by the small portion deflected into the observed present orbit of such a comet or meteoric flight. We cannot escape the difficulty by supposing the whole mass of a comet to arrive in the form of a cluster much smaller than the head of any known comet; for in that case, though the whole comet would be captured, yet it would be captured in the form of a cluster far too compact to undergo such subsequent dissipation as we must of course account for in the case of every one of the known meteoric flights. Nor could the head of a comet, supposed to be a tolerably dense and massive body, by passing at the right distance from a giant planet, be properly deflected with its whole company of meteoric attendants, except by assuming that the head had such power by virtue of its mass as would effectually prevent its satellite meteors from ever escaping from its control, which they must do before they could extend themselves along hundreds of millions of miles of its track, as we see in the case of such meteor families as those which produce the November and August showers of falling stars.

Failing this explanation, astronomers have found themselves almost compelled to adopt the theory, wild though it seems at a first view, that those comets and meteoric systems whose paths pass very near the track of a planet must at some remote epoch have actually been expelled from the interior of the planet when that orb was in a sun-like state. Possibly a theory so startling might not have suggested itself, even in presence of evidence which appeared to leave no other available explanation of relations unmistakably existing had it not been that a number of circumstances had combined to suggest that many of the larger meteoric masses which have from time to time fallen upon the earth have been expelled from the interior of the sun or of some one or other of his fellow suns, the stars. The microscopic structure of meteorites shows that they were once in a state of intense heat such as exists only in the immediate neighbourhood of suns, if even anywhere save in their interior. The chemical analysis of some meteoric masses has indicated the presence of larger quantities of occluded hydrogen than could (it would seem) have attained that condition except under the enormous pressure prevailing in the interior of a sun. Then the evidence of solar eruptions driving matter from the sun with a velocity so great that such matter would never return to him—his power of recalling matter expelled from his interior being limited to the control of bodies whose velocity when leaving his surface did not exceed 360 miles per second—suggested the existence of similar power in all suns. And other evidence might be cited, did space permit, in favour of the theory that not only have some meteors which reach the earth been expelled from suns or stars, but that even now these suns continue to expel matter from time to time with such velocities that the expelled matter forthwith starts on a journey through interstellar space, a journey not to cease until, after uncountable ages, such matter shall fall on some other sun (perhaps after multitudinous flittings from system to system) or on a planet circling around such a sun. Now, the theory is generally accepted by astronomers of the present day, that every orb in a system like our solar system, even though now dark like our earth, or cold and in a sense dead like our moon, passed through a sun-like stage, when large portions of its mass were vaporous with intensity of heat. In this stage (which possibly some of the giant planets have not so very long since passed), they would expel matter from their interior from time to time, just as suns now do, according to the theory we have just considered. Now, their expulsive force would of course be much less than the sun's; for indirectly, though not directly, this power would depend almost wholly upon the total mass or quantity of matter in a sun-like body. But so far as their power of expelling matter never to return to them was concerned, the giant planets—Neptune and Uranus, Saturn and Jupiter—would be not inferior to the sun himself, since the velocity which one of these planets would have to communicate to expelled matter, that it might for ever be freed from the planet's influence (unless chance brought such expelled matter and the expelling planet back after many revolutions of both to the scene of the original catastrophe, when the planet might gather back the matter it had so long before driven forth from its interior), would be much less than that which a sun must give to erupted matter to render it similarly free. In fact a planet would in some degree have an advantage over a sun, since matter expelled to a great distance from a planet would forthwith be under the influence of the sun round which that planet was travelling, and would so travel in an independent orbit even though the original eruptive action had not communicated to the expelled matter the full velocity necessary to free it from the parent planet if no other orb existed in the universe.

Accordingly, most astronomers who have carefully considered the matter have been led to regard the theory as far more probable which considers the November meteors—to take that system as a convenient illustrative case—to have been originally expelled from Uranus, than the theory which supposes meteors travelling originally amid interstellar space, to have accidentally passed so very near Uranus that his perturbing influence entirely changed the character of their orbit.

But so soon as we recognise that a planet like Uranus would be able to eject matter from its interior as effectively as the sun, or even more readily, we perceive that what is true of the giant planets must be true of smaller planets, like our own earth, for instance, or Mars, or even of such bodies as the moon, the satellites which attend on Jupiter and Saturn, the asteroids, and even smaller bodies. In passing, indeed, we may notice that the truth of this theory with respect to such small bodies as aerolites is often illustrated in a very striking manner in our own skies. For whenever one of these bodies is caused by friction with our atmosphere to assume the sun-like condition —that is, to become intensely luminous—we see that it scatters fragments from its own mass, on all sides, and certainly these fragments are not gathered up again by its own attractive energy. So that we might almost be led to infer that the smaller any orb in space may be, the more likely is it, when passing through the sun-like stage, to eject portions of its mass. Without insisting, however, on this conclusion, we may at least consider ourselves free, should other circumstances point that way, to adopt for any meteoric system not explicable as expelled from a giant planet, the theory that the system was at some remote epoch expelled from a smaller planet, a moon, or an asteroid.

It is to a theory of this sort that Dr. Ball, Astronomer Royal for Ireland, has been led by the study of the relations presented by certain meteorites. These relations may be thus presented (we slightly modify Dr. Ball's words): Meteorites are always angular fragments, even before they reach our air. Many meteorites have a crystalline structure, and according to Haidinger, this indicates a very long period of formation at a nearly constant temperature—a condition which can only be fulfilled in a large mass. In other meteoric stones many fragments are welded together, as in the terrestrial formations called breccia. Other meteorites are composed of very small particles, analogous to volcanic tufas.[1] Many meteoric stones show markings, resembling those seen on terrestrial rocks, and caused by the rubbing together of adjacent masses.

These features were first noticed by Tschermak, in his interesting memoir on the structure of meteorites; and, referring to that paper, Dr. Ball remarks that although he does not feel competent to offer an opinion on the mineralogical questions involved in the discussion, the numerous arguments adduced by Tschermak seem in his (Dr. Ball's) opinion, to justify the conclusion that the meteorites have had a volcanic source on some celestial body. 'We may suppose,' Tschermak had said in conclusion, 'that many celestial bodies of considerable dimensions are yet small enough to admit of the possibility that projectiles driven from them in volcanoes shall not return under the action of gravity: these would really be the source of meteorites.' Similar views have been advanced by Mr. J. Lawrence Smith, and others, who have given considerable attention to the subject. Wherefore, Dr. Ball considers that it is not unreasonable to discuss the following problem: —'If meteorites have been projected from volcanoes,

[1] The name *tufa*, from the Italian *tufo*, porous ground, is given to certain porous loose rocks, sometimes calcareous, and sometimes composed of fine powdery volcanic dust, more or less completely cemented by the infiltration of water, but generally loose and spongy. It is to tufa of the latter kind that the substance of some meteors seems to be analogous. The dust of such tufas consists chiefly of material ejected from volcanoes, a circumstance on which a part of Dr. Ball's reasoning will be found to depend.

on what body or bodies in the universe may these volcanoes have been located?'

He begins first with the sun. 'It has been abundantly shown,' he says, 'that there exists upon the sun tremendous explosive energy. It is not at all unlikely that the energy would be sufficiently great under certain circumstances actually to drive a body from the sun never to return. We might, therefore, find upon the sun adequate explosive power for the volcano: but the projectiles are here the difficulty. There are a number of circumstances (notably the breccia-like appearance of some meteorites) which show conclusively that the meteorites have been torn from rocks which were already nearly, if not quite, solid; and, as it seems in the highest degree impossible that rocks of this nature should exist in the sun, we may conclude that the sun has not been the source of meteorites.' Here it must be remarked, first, that the objection applies only to those meteorites which present such appearances as to compel us to believe that they were torn from rocks nearly or quite solid, so that the general statement that 'the sun has not been the source of meteorites' is not established by the evidence. Secondly, however, it is worth considering whether the sun-like stage of a celestial orb is after all that in which the ejecting power of the orb would be most freely developed. May it not be absolutely essential, indeed, to the full ejective activity of such an orb that a solid crust should have formed over the greater part of its surface?

Next, Dr. Ball inquires whether 'meteorites' (but it must be borne in mind that certain orders only of meteorites are really in question) 'can have come from the moon.' 'Owing to the small mass of the moon,' he says, 'the explosive energy required to carry a body away from the moon is comparatively small. Can such a body fall upon the earth? To simplify questions of the kind, we shall suppose various disturbing influences absent. We shall suppose that the projectile is discharged from a volcano in the moon with sufficient velocity to carry it therefrom. We shall then omit all account of the disturbing influence both of the sun and the moon on the projectile, and we shall suppose that the projectile is really revolving round the earth as a satellite;' or, as the rest of the argument requires, that the projectile begins to revolve in this way. Then he shows that, as is indeed obvious, the projectile will fall on the earth if its course when once fairly started from the moon gives to it an orbit intersecting the earth, on passing nearer to the earth's centre than a radius of the earth. And clearly, apart from disturbing influences, if the orbit does thus intersect the earth's globe, the projectile will finish its career as a free traveller before it has traversed quite one half of a complete orbital revolution round the earth; while, if the orbit does not intersect the earth, the projectile will travel for ever round and round its orbit without falling upon the earth. Consequently, lunar projectiles cannot now fall upon the earth, unless the lunar volcanoes are still active, which certainly is not generally the case, and most probably is not the case even with a single lunar volcano. 'It is generally believed,' says Dr. Ball, and he might as truly have said 'it is certain,' 'that lunar volcanoes are not now active to any appreciable extent, even if the suspected indications of recent change were thoroughly established.' Meteoritic masses may have been expelled from the moon in remote times, and may still continue to travel around the earth, while, again, the orbits of such masses may occasionally be caused by perturbing action to intersect the earth, so that the lunar meteorite is caused to enter our atmosphere, and to fall upon our earth's surface. But such cases must be few and far between, and certainly quite too infrequent to account for any but a very small proportion of the meteorites we are considering. Dr. Ball next considers the planets, and in order to get over the difficulties of the great initial velocity which would be necessary to overcome the gravitation of a large planet, he inquires if a volcano placed upon one of the small planets could accomplish the task. There is no real reason, however, for thus limiting the inquiry, seeing that, as we have already pointed out, the eruptive energy of a still youthful planet—a planet, that is, in the intensely volcanic era of its existence—would depend in the main on the quantity of matter in the planet, precisely as the velocity necessary for the complete rejection of matter would depend on the same relation, so that large and small planets would probably be on about an equal footing in this respect. Indeed, so far as the total quantity of ejected matter was concerned, the larger planets would supply far the larger portion of the meteoric masses now travelling freely about the solar system, for the simple and sufficient reason that the matter-rejecting era of a large planet would certainly last much longer, while the quantity of matter ejected in any given time would probably be much greater than in the case of a small planet. However, Dr. Ball's arguments are not specially affected by this consideration, and having premised so much we may leave the reader to apply to the case of a giant planet, with suitable modifications, the reasoning which Dr. Ball appears to limit to the case of one of the minor planets or asteroids.

He considers 'the circumstances under which it would be possible to discharge a projectile from the surface of a planet—say Ceres—so that the projectile shall intersect' the ring of space, between 8,000 and 9,000 miles wide, which the earth's globe traces out year after year around the sun; for in this case only can it happen—and in this case it may happen or may not—that the earth and the meteorite may meet at the intersection of their paths, the long travels of the meteorite being thus brought to an end. 'The planet being small,' he proceeds, 'the initial velocity that would be required to carry a projectile from its surface presents no difficulty: perhaps an ordinary cannon would be sufficient so far as the mere gravitation of the planet is concerned.' But, of course, this would not be sufficient. A projectile started from Ceres with such a velocity, although it would perhaps never return to Ceres, would travel round the sun in an orbit scarcely differing appreciably from that of Ceres, and thus would never approach within many millions of miles of the earth's orbit. Herein, indeed, lies the great difficulty in the case of a small planet. The expulsive energy necessary to cause a projectile to travel on a path intersecting the earth's exceeds not merely by a large absolute amount, but *manifold*, that which would be required merely to overcome the gravitating power of the planet itself. In the case of a giant planet the power required to send a projectile on an orbit intersecting the earth's would still largely exceed that necessary merely to free the projectile for ever from its parent planet; indeed, the excess would be *absolutely* greater in most cases than it would be for a small planet like Ceres travelling much nearer to the earth's orbit; but as compared with the force necessary to overcome the planet's own gravity, the excess in the case of a giant planet would be much smaller than in the case of Ceres or any of the planetoids.

Dr. Ball enters into the calculation for Ceres, regarding this planet as moving in a circular orbit with a velocity of about eleven miles per second. He shows that a volcano on Ceres, to eject a projectile which might encounter the earth, must be at the least capable of producing an initial velocity of three miles per second. 'As this is quite independent of the additional volcanic power requisite to carry the projectile away from the attraction of Ceres, it is obvious,' proceeds Dr. Ball, 'that after all there may be but little difference between the volcano which would be required on Ceres, and that (of six-mile power) which would project a body away from the surface of the earth for ever.'

But, even supposing there existed on Ceres, or on any or all of the minor planets, volcanoes of sufficient power to eject projectiles with such velocity that they might cross the earth's track, the question still remains whether any considerable proportion of them would do so. Dr. Ball deals with this question in the following form:—'Suppose a projectile is discharged from a point in the orbit of Ceres' (that is, from Ceres) 'in a random direction, with a total initial velocity of twelve miles per second, determine the probability that the orbit of the projectile will cross the earth's track.' The solution of this problem, though not very complex in reality, would not be by any means suited to these pages. The result, however, is sufficiently simple, and exceedingly significant. It appears that the odds are about 50,000 to 1 against one of these projectiles crossing the earth's track. In other words, for every one of those projectiles which cross the earth's track, 50,000 or thereabouts must have been ejected. As the total number of meteorites whose paths cross the earth's track enormously exceeds the total number which have been actually encountered by the earth, it follows that we should have to imagine the ejection of millions of millions of meteorites from the asteroids before we could adopt the theory that it is from those bodies the meteorites really have been derived. The argument is increased in strength when we consider the case of a giant planet, for the farther away any planet is from the earth the smaller is the probability that a projectile, even if ejected with sufficient velocity to come nearer to the sun than our earth is, will actually cross the earth's track. Of course the circumstance that some systems of shooting-stars actually have orbits crossing the earth's track while extending farther into space than the orbit of Uranus—in some cases farther even than the orbit of Neptune—is in itself a sufficient answer to any objection implying the impossibility that projectiles expelled from Jupiter, Saturn, Uranus, and Neptune should cross the earth's track. But the general objection remains valid—if we are to suppose that *all* shooting-stars, meteorites, and aerolites have come from the planets of the solar system, we must assume that the volcanic activity of the planets has been enormously developed, since, first, we have not seen one member of many millions belonging to any known meteoric system, and, secondly, the meteoric systems of which we know anything form but a mere fraction of those of which (owing to their position in space) we *may* learn something, while, thirdly, these are but the smallest fraction of those which actually exist—to say nothing of the enormously long time-interval during which meteors of all orders have been gathered up by the earth with none to note the process.

Thus we are led to inquire whether some, at any rate, of the meteorites may not have come from a source which might have ejected meteoric matter under more favourable conditions for subsequent capture by the earth.

At this stage of the inquiry Dr. Ball adopts quite a poetical, one may even say a dramatic, method of dealing with his subject. He no longer speaks of this or that planet by name, but describes the qualities of one particular planet, whose position in the solar system the reader is left to infer from his description. 'There is one planet of the solar system,' he says, 'which has a special claim to consideration. On that planet it is true that a volcano would be required which was capable of giving an initial velocity of at least six miles per second; but every projectile launched from that volcano into space would, after accomplishing its elliptic orbit round the sun, dash through the track of the earth, and again pass through the same point at every subsequent revolution. It is not here a case of one solitary projectile out of 50,000 crossing the earth's track, but every one of the 50,000 possesses the same property.' Where, it may be asked, is this specially favoured planet whose meteoric projectiles thus inevitably intersect the track of the earth? We have not far to look for it; it is the earth itself on which we live. The earth is certainly not now able to expel meteors with the velocity required by this theory, or, as the present writer has said elsewhere, 'if capable of so doing, she (fortunately perhaps for us) refrains from exerting her full powers in this way.' But in the remote past, as we have every reason to believe, the earth possessed much greater volcanic energy than she now does. 'If in ancient times,' says Dr. Ball, 'there were colossal volcanoes on the surface of the earth which had sufficient explosive energy to drive missiles upwards with a velocity sufficient to carry them away from the earth's surface, after making allowance for the resistance of the air, these missiles would then continue to move in orbits round the sun, crossing at each revolution the point of the earth's track from which they were originally discharged. If this were the case, then doubtless there are now myriads of these projectiles moving through the solar system, the only common feature of their orbits being that they all intersect the earth's track. It will, of course, now and then happen that the earth and the projectile meet at the point of crossing, and then we have the phenomenon of the descent of a meteorite.' Dr. Ball goes on to remark that this theory was, so far as he knows, first put forward by Dr. Phipson, a statement which at first sight seems abundantly justified by the following passage in Dr. Phipson's useful compilation, 'Meteors, Aerolites, and Falling Stars':—'If in future years, extended observation enforce more and more upon us the truth of the assumption that meteorites are really *the dust of the earth*—fragments of the earth's mass thrown from it in its early years (in the infancy of the globe, when volcanic action was intense; probably long after the moon was separated from it), which myriads of fragments have continued ever since to circulate along, or near to, the earth's path—then I shall be satisfied to have originated this theory.' But Dr. Phipson's theory is in reality entirely different from Dr. Ball's. The orbits he assigns to the expelled meteorites are not orbits round the sun, but orbits round the earth—a thing not only entirely different in character, but standing on an entirely different scientific footing—if it ought not rather to be called entirely unscientific, as compared with the truly scientific theory propounded by Dr. Ball. 'We know a planet—Saturn,' says Dr. Phipson, 'surrounded by several rings which undergo slight perturbations only; and taking especially into consideration the chemical composition of aerolites, we may be tempted to suppose that these meteoroids have orbits *round the earth'* (the italics are his), 'not round the sun, and that they constitute a series of *dark rings* round our globe, similar perhaps to the rings of Saturn.' He proceeds to enforce this theory (though his arguments are not in reality so valid as he supposes), speaking of it as the satellite theory, up to the passage quoted above, the sentence immediately preceding which (except one referring to later chapters) runs thus:—'After what has been already said, the reader will be able to form his own notions, and to choose between the satellite and planetary theories of meteoroids.' Science has long since done so, and has definitely adopted the planetary theory, of which general theory Dr. Ball's indicates but a special case. The satellite theory is, in fact, utterly untenable, for the simple reason that a projectile expelled from the earth so as to remain an attendant of the earth would return to the earth before completing one entire revolution. On the other hand, Dr. Ball's views are entirely in accordance with scientific possibilities, and seem so well to correspond with the observed peculiarities of certain meteorites, that it must be regarded as extremely probable that they are just, though it can by no means be admitted that they account for all meteoric systems, or indeed for those, like the November and August systems, about which astronomers have learned most.

It is noteworthy that almost simultaneously with the enunciation by Dr. Ball of the theory we have been considering above, the Paris Academy indicated its recognition of the labours of M. Stanislas Meunier's researches into the structure of meteorites. Astronomers and physicists had taken great interest in the labours of Daubrée, indicating a connection between meteorites and the lower strata of the earth. M. Meunier, who may be regarded as Daubrée's pupil and follower, has found that this analogy is not confined to mineralogical constitution, but appears to extend also to the relation which these cosmical materials present when they are compared together as we compare the constituent rocks of our earth. His conclusion is somewhat startling; and even the support his views have derived from the recognition of the Paris Academy will scarcely justify us in regarding M. Meunier's theory as demonstrated by the evidence:

he infers that all the meteorites 'once belonged to a considerable globe like the earth, having true geological epochs, and that later this globe was decomposed into separate fragments under the action of causes difficult to define exactly, but which we have seen more than once in operation in the heavens themselves.' He refers doubtless to the phenomena presented by the so-called 'new stars.' It is rather a bold assumption, however, that the blazing forth of a new star indicates a process under the action of which a globe has been decomposed into separate fragments. (If by any chance he refers to any other celestial phenomena, then all we can say is that a somewhat wide reading respecting astronomical matters has not yet brought under our notice any phenomena which could be so interpreted.) But it seems to us that, if Dr. Ball's theory be adopted, we have an answer to the otherwise rather puzzling question, what that globe can possibly have been from which the fragments representing successive geological eras have reached our earth during countless millions of past ages. As we have elsewhere pointed out, 'Stanislas Meunier's theory, as it stands, is preposterous, let Commission or Academy say what they will. That some other planet (for so he presents his theory) has been torn into fragments, millions of which have in successive eras reached our earth, their constitution varying according to the depth of the strata of the planet home from which they were successively torn, is a theory utterly inadmissible so long as the laws of probability are to be our guide in such matters. But that the earth herself in various past stages of her existence as an intensely volcanic orb, should have expelled immense numbers of bodies, and that the successive periods of meteoric downfall should thus come to exhibit changes corresponding to the successive stages of terrestrial stratification, seems reasonable enough. Nay, we may even say that if many meteorites really are proved by the evidence adduced by Tschermak to have had a volcanic origin, no theory but Dr. Ball's will account for *those* meteorites at any rate, while nothing could accord better than this theory with the results of M. Stanislas Meunier's researches.'

But now let us examine the conclusions to which we seem led by the evidence respecting falling stars, meteors, and aerolites. These are not nearly so simple as might be imagined by those who examine merely the results of researches which have led to the formation of special theories. When we read what Schiaparelli, Hoek, Leverrier, and others have written respecting star-showers, we might be led to believe that all the phenomena presented by those bodies can be accounted for by what may be called the interstellar theory: the theory, namely, that all meteor systems existed originally as clouds of meteoric matter, travelling amid interstellar spaces, whence they were drawn by the attraction of our sun toward the solar system, in approaching which they were so disturbed by the attractions of some planet that thenceforth they have travelled in a closed curve, instead of returning to the interstellar depths after making their perihelion swoop around the sun, as in the ordinary course of things they would have done. If we limited our reading to the results obtained by Professor Graham, in the chemical analysis of certain meteors, and to those results of microscopical investigation which seem to support Graham's views, we might infer that all meteors were originally expelled from the interior of bodies like our sun. This theory, extended to include the giant planets, as formerly minor suns, would go far to explain most of the phenomena presented by meteors. But we have seen that from the study of some meteorites Tschermak, Ball, Lawrence Smith, and others, have been led to advance the general proposition that meteorites were originally earth-born. Yet again those who, like the present writer, regard the theory that the solar system was formed by processes of aggregation as preferable to the so-called nebular hypothesis (which regards the solar system as formed by the contraction of a great mass of gaseous or cloudlike matter), or rather who consider that the nebular hypothesis must be supplemented by such a theory, might be disposed to regard meteors and aerolites as the fragments left after the system had been formed, and to find an explanation of all the principal phenomena of meteoric systems in the results of such processes of aggregation continued until nearly the whole of the matter available for the formation of the solar system had been gathered in. How are we to select from among so many seemingly conflicting theories, for each of which a considerable amount of evidence may be adduced? or, if selection is impossible, how can we either reconcile them as all true, or find some better theory, which may enable us to regard them as all false ?

It has long seemed to us that, in dealing with subjects so complex as this, it is unwise to limit our attention to a single theory, or rather (for it is thus that a single theory comes to be advocated as the only available one) to one special section of the available evidence. We must endeavour to attach due weight to all the known facts, not to consider those only which suggest or support some favourite view. In the present case we shall be led, when this is done, to admit that most of the theories above referred to are so strongly supported that, instead of attempting to select among them, we ought to endeavour to show rather how they may all be accepted. Here, of course, we do not refer to theories like the satellite theory of meteors which could only be supported by persons ignorant of the laws of motion. We know that, on the one hand, matter expelled from the earth never could have formed a ring of meteors round the earth, while, on the other hand, a ring of meteors round the earth never could account for the downfall of meteors upon the earth. And although Schiaparelli's theory of the origin of meteor systems stands, of course, on a very different footing, Schiaparelli being himself a mathematician of considerable power, while his theory has received the support of mathematicians of first-rate abilities, yet it appears to us that when the considerations indicated above are fairly taken into account, this theory must be rejected as inadmissible. But all the remaining inferences of those mentioned in the preceding paragraph are supported by evidence so strong that we cannot readily reject them. It is as nearly certain as a matter of this sort could well be, that a number of the meteorites which fall from time to time upon the earth have been expelled from planets or from a planet having already a solid crust, and the only explanation which seems admissible, so far as such meteorites are concerned, is that they were expelled from our own earth in some remote stage of her existence. Again, whether we trace back the history of the earth by examining the various strata forming her crust, or whether we consider the evidence afforded by the condition, orbital movements, &c., of the solar system, we are alike led to the conclusion that every planet has in the remote past been in a state of intense heat, and that therefore presumably what happened to our own earth must have happened to all the planets, so that the very evidence which supports so satisfactorily the theory of Ball and Tschermak, conducts us also to the conclusion that immense numbers of meteorites must have been expelled from every member of the solar system (unless we exclude the giant planets on the ground that as yet they may not have attained the stage of effective volcanic eruptive action), and therefore that some at least among the meteorites which reach us must have come from other worlds than ours. As it is exceedingly unlikely that the giant planets are as yet so youthful as the exception just suggested would imply, and as the total expulsive action of a planet must be in some degree proportioned to the planet's mass, it would seem probable that large quantities of meteoric matter must come to us from Jupiter, Saturn, Uranus, and Neptune, even if we had no direct evidence of this in the circumstance that so many meteoric systems have orbits carrying them to and beyond the orbits of those giant orbs. But this is not all. The evidence showing that the solar system has been formed by processes of aggregation, although it may be insufficient to establish the theory that aggregation rather than contraction has been the effective process, yet suffices to show that each planet has gathered in no small portion of its entire mass from without. Now, if we consider what, under these conditions, would be the present arrangement of meteoric and cometic systems remaining after the progress of aggregation had been continued almost to its close, we perceive that some at least among these systems would have precisely such positions as we recognise among the known meteoric and cometic systems. A nebulous mass which had just escaped capture in the process of aggregation would thereafter travel on an orbit passing very close to the orbit of the forming planet which had failed to effect the capture of the mass. And we could readily understand that in the earlier condition of a planet—that is, when its whole mass was vaporous, and therefore enormously expanded—it would have had a much better chance of effecting such partial captures than in its later condition as a cool condensed globe. (We say partial capture, for it must be remembered that, although in such a case a nebulous mass would not there and then become part of the mass of the planet, it would for ever thereafter travel on an orbit intersecting the planet's, and in the long run could not fail to be captured piecemeal, though countless ages might be required for the purpose,[1] were it not that the perturbing influences of other members of the solar system might so change the orbit of the nebulous mass that it would pass free of the planet's course.) So that the total number of meteoric systems which we might expect to result from the breaking up of such partially captured nebulous masses would be much greater than could by any possibility be captured in the way suggested by Schiaparelli.

But, passing from the consideration of the various theories which must be taken into account in any complete discussion of meteoric relations, let us study some

[1] Simply because to capture a fragment of the nebulous mass before this had become greatly extended, the planet must pass the point of nearest approach of the two orbits when the mass happened also to be there, which might not happen once in the course of many revolutions of both bodies. On the other hand, when the nebulous mass had become greatly extended (as the August and November meteoric systems have become), although encounters would be more numerous, the quantity of matter captured at each encounter would be very small. We have spoken a little later of the possibility that perturbations might so change the orbit of the nebulous mass (regarding it as a whole) that it would pass clear of the orbit of the planet; but it should be noted that the effect of such perturbations would be oscillatory, the mean distances of the orbits remaining constant when long periods of time are taken into account.

of the thoughts suggested by the theory which forms the more especial subject of this essay—a theory, be it remembered, which must be regarded as to all intents and purposes established by the evidence, though not as the sole theory in explanation of meteoric phenomena.

In the first place, it should be noticed that the time intervals over which our thoughts must range in considering this theory of meteorites, although not quite so great as those involved in some astronomical theories, are nevertheless enormous. The mere fact that so many hundreds of thousands of these earth-born meteorites have been in the first place strewn around the zone along which the earth pursues her course, and then gathered up by the earth (so far as they have as yet been gathered up), would of itself demonstrate the lapse of many millions of years since the former process began. For, although the earth must of necessity, as we have seen, pass always either through or very near the orbit pursued by each meteorite expelled from her interior (through the orbit before disturbing attractions had affected its shape, and near the orbit even when such attractions had produced their greatest effect on one side or on another), yet, in most cases, many circuits of the earth—that is, many years—would elapse before the earth and an earth-born meteorite would again be simultaneously near the scene of the original outburst which gave the meteorite separate existence; thousands of years would elapse (on the average) before an approach close enough, apart from perturbations, to bring the meteorite to rest upon the earth would occur; and the chances would be enormous against the occurrence of one of these near approaches at a time when the meteorite's orbit was, at this point, in actual intersection with the earth's. Perturbations would sway the meteorite's orbit and also the earth's orbit hither and thither across the mean position of either —not to any great extent, considering the dimensions of the solar system, but by a range amply sufficient to separate the point of nearest approach of the two orbits more than a diameter of the earth from each other. So that unless a close approach of the earth and meteorite occurred at a time when in the swaying hither and thither of the two orbits the effect of perturbations at the place of nearest approach of the orbits was nearly at a minimum, the earth and meteorite would pass clear of each other, however nearly the two might synchronise in their passage of the respective points where the two orbits at the moment approached each other most nearly.[1] Thus we recognise in the

[1] The non-astronomical reader will find some difficulty in understanding the above sentence, if he does not note carefully the distinction between the close approach of two orbits and the close approach of two bodies travelling in those orbits. The orbits, undergoing constant flux, may approach each other very closely at some point, or may even intersect at a moment when the bodies travelling on those orbits are very far apart; and *vice versa*, the two bodies may make a near approach to each other by coming nearly simultaneously to the points where the two orbits approach most nearly, yet at the moment the orbits may *there* be separated (owing to perturbations) more widely than usual. For a very near approach of the two bodies, both conditions must be simultaneously fulfilled: the points of nearest approach of the two orbits must be brought by perturbations very close together, and the two bodies must reach those points very nearly at the same time.

myriads of meteorites which have already been gathered in, and in the circumstance that as yet the supply shows no sign of exhaustion, conclusive evidence that millions on millions of years must have elapsed since first such meteorites were expelled from the interior of the still youthful earth.

But we may carry back much farther the range of our mental vision. The meteorites we are considering present clear signs, as has been shown, of having once formed parts of solid strata, and not only so, but of strata which must have been formed slowly. We thus recognise the co-existence during a long time-interval (a period itself measurable probably by myriads of years) of two features which we have been apt to regard as belonging to different eras of the earth's history —a solid crust and an explosive energy competent to expel matter so forcibly that thereafter it would be free from the earth's control, though not from accidental future encounters with the earth.

But once again we are thus led to recognise the prior existence of yet longer periods, when the greater part of the substance of the forming earth was vaporous, when in fact during the process of slow contraction the earth was gathering, as it were, those powers by which during the sequent stage of her existence she was able to expel millions of meteoric masses from her interior.

Even more interesting, however, than the considerations thus suggested as to the past stages of our earth's history, is the thought that what happened to our earth must have happened to all the planets of the solar system—nay, we may say almost certainly, must have happened, or must be now in progress, or must happen hereafter, with every orb throughout the infinities of space. Each sun and each planet, each asteroid and each moon (to say nothing of nebulæ on the one hand, or of comets and aerolites on the other) has its eruptive stage, in which, diverse though the powers of large and small orbs may be, expulsive power probably has been, is, or will be attained, competent to drive the expelled matter beyond the attractive range (also diverse for orbs of different size) of the parent mass. Nor need we be perplexed by the consideration that, in thus viewing millions of meteors and meteorites as sun-expelled or planet-expelled masses, we seem to set on one side the evidence which shows that the orbs peopling space have been in large part formed by the aggregation of meteoric masses. The two processes are no more inconsistent than are the two processes by one of which trees gather nutriment from the earth, and so grow, bud, blossom, bear fruit, and throw out leaves, while by the other they strew upon the earth leaves, fruit, blossoms, and buds, and in the fulness of time yield even their own substance to the all-nourishing soil. The earth-born meteorites which return in thousands year by year to the earth from which they sprang are but as the leaves of a tree compared with the soil from which the tree derives its nourishment, when we compare the total mass of all those meteorites with that of those portions of the mighty cosmical nebula from which the mass of the earth itself was formed; while this portion in turn compared with the whole nebula is but as the soil nourishing a single tree to that from which a whole forest derives support.

THE ARCHITECTURE OF THE UNIVERSE.

The noblest and the most difficult of all the subjects with which astronomy has to deal is the problem of determining the true structure of the universe. 'A knowledge of the construction of the heavens,' said Sir W. Herschel toward the close of his career as an observer, 'has always been the ultimate object of my observations.' Yet few of the astronomers whose names stand highest in the roll of fame, and still fewer of those who have held official rank in the science, have given close attention to this subject. The problems involved are indeed too difficult to be solved by the ordinary methods of astronomical observation or calculation. The exact details, for determining which great public observatories have been formed, are of little use in the inquiry. The stars must be dealt with, not individually but by thousands and tens of thousands, if the galaxy is to reveal its secrets. Such methods as Sir W. Herschel employed in dealing with the problems of the universe appear rough, no doubt, and inexact, compared with the systematic study of star-positions, carried out by official astronomers, to subserve terrestrial purposes. But finer researches, more minute and detailed observations, fail utterly when applied to the mighty proportions of the universe revealed by the telescope. We must draw broad and strong lines in endeavouring to picture the galaxy. If we attempt to refine, the picture becomes meaningless.

It is probably on this account, and because detailed observations are in themselves more easily comprehended, while their specific value in terrestrial applications of astronomy is altogether greater, that so few astronomers have discussed the problems presented by the structure of the universe. Not only have astronomers seemed unwilling to undertake the observations necessary for the purpose, but they have not been careful even to investigate the observations made by others. We speak without exaggeration, and indeed from our own knowledge, when we assert that very few of the chiefs of the leading European observatories at the present time have mastered the full scope and bearing of the wonderful series of papers in which Sir W. Herschel presented the results of his researches into the star depths between the years 1784 and 1818—that is, from the year when he first indicated his intention of dealing with the great problem, and that in which he offered his last observational contributions toward its solution. We would not say that the official astronomers of Europe actually accept the absurd travesty of Herschel's researches presented in text-books (for which no less eminent an astronomer than Arago is mainly responsible), but it becomes manifest, whenever they refer to the work of Sir W. Herschel, that they confuse his earlier views (or rather fancies) with the results to which he was led by his actual researches, and regard the latest ideas thrown out by him as worthy of equal consideration with the opinions he definitely indicated as based on observation. The late Wilhelm Struve, of all the really eminent official astronomers of the present century, after adopting for many years the vague ideas respecting Herschel's work thus commonly entertained, was led by the careful investigation of a complete series of W. Herschel's papers presented to him by J. Herschel, to the conclusion that hitherto he had wholly misapprehended the views of the great astronomer of Slough. It was then that Struve asked, with some surprise, 'Pourquoi les astronomes ont-ils maintenu généralement l'ancien systéme énoncé en 1785, quoiqu'il eût été entièrement abandonné par l'auteur lui-même?' Even he, however, though perceiving that the earlier views of Herschel had been abandoned, did not sufficiently distinguish between Herschel's later *opinions* and *ideas*—that is, between views to which Herschel was led by his observations, and ideas which he regarded as worthy of being tested by observation but had not actually so tested.

We propose in this essay to indicate the actual range of W. Herschel's work on this problem of the structure of the universe, carefully defining what he proved, disproved, and conjectured; as also the methods he employed, which, while altogether diverse, have been as completely confounded together as his theories and speculations. We shall then consider the bearing of later researches, including our own, on this problem of the structure of the universe, and indicate the line on which, as we think, the problem may be most successfully attacked hereafter. For at the outset we wish to impress upon the reader the fact that, though some outworks have yielded to the attacks of astronomers, the great problem itself still remains unmastered. It would be more agreeable, doubtless, if we could enunciate definite opinions, and clearly say how the universe of stars is formed. But it was only when the true complexity of the problem was as yet unrecognised that this could be done. Then, as W. Struve has well pointed out, a complete system could be advanced. Sir W. Herschel's original theory, for example, as advanced in 1785, was 'un systéme entier, imposant par la hardiesse et la précision géométrique de sa construction.' Whereas, proceeds Struve, 'dans ses traités publiés depuis 1802, on ne rencontre que des vues partielles.' But, as we can make no question that, imperfect and partial though the later views of W. Herschel were, they were more exact and trustworthy than the fuller system of 1785, so it must be admitted that it is a better preparation toward the final solution of the great problem of the universe to eliminate what is unknown and doubtful, than to pretend to advance a complete theory while as yet the evidence for forming one is insufficient.

Very little was done before W. Herschel's time to master this problem by observational or other researches. Yet it would not be fair to pass over the work of

Wright, Kant, Lambert, and Michell. Wright, by means of a very small telescope satisfied himself that the Milky Way consists entirely of stars, and adopting the theory that the stars are arranged with a certain general uniformity throughout the galaxy, he was led directly by the observed richness of stars on the zone of the Milky Way to the theory commonly attributed to W. Herschel and known as the Grindstone Theory. This he presented in the following terms: 'If we suppose the sun to be plunged in a vast stratum of stars, of inconsiderable thickness compared with its dimensions in other respects, it is not difficult to see that the actual appearance of the heavens may be reconciled with a harmonious arrangement of the constituent bodies of such a system. . . . It is evident that the stars would appear to be distributed in least abundance in the opposite directions of the thickness of the stratum, the visual line being shortest in these directions; and that the number of visible stars would increase as the stratum was viewed through a greater depth, until at length from the continual crowding of the stars behind each other, it would ultimately assume the appearance of a zone of light.'[1]

It was in 1750 that Wright advanced this theory. In 1755, Kant (who in 1751 had read a translation of Wright's essay printed in a Hamburg journal) advanced a theory which in parts corresponded very closely with Wright's. He extended the reasoning, as Wright had

[1] From an abstract of Wright's paper in Professor Grant's excellent work, *A History of Physical Astronomy.*

already done, to the few nebulæ then known, which he regarded as external galaxies similar in dignity in the scale of creation to the galaxy of which our sun is a member. He conceived also the thought that the system of galaxies thus indicated formed but the third term of a series of worlds and systems, satellite systems and solar systems being the first and second terms—'and these first terms of an infinite series,' he proceeds, 'enable us to infer the nature of the rest of the series.[1]

Lambert advanced a theory unlike Wright's and therefore unlike Kant's in several important points—though Kant remarked, in 1763, that the accordance between Lambert's views and his own 'extended even to the most minute details.' In Lambert's theory solar systems were regarded as of the first order. He considered that our sun belongs to a vast globular group or cluster of suns, forming a system of the second order, which according to his view includes all the scattered stars not belonging to the Milky Way. He maintained that many systems of the second order combine to form a system of the third order, this system being so shaped (in the case at least of the group of clusters to which our sun's cluster belongs) as to form a stratum, the concourse of clusters in the direction of the greatest range of the system forming the milky light of the galactic zone. He then proceeded to consider the probability that other systems of the third order exist,

[1] 'Allgemeine Naturgesichte und Theorie des Himmels; oder Versuch von der Verfassung und dem mechanischen Ursprunge des ganzen Weltgebundes nach Newton'schen Grundsätzen abgehandelt.'

forming together a system of the fourth order, and he touches on the inference that there may be systems of higher orders *ad infinitum*. This theory was manifestly altogether distinct from Kant's, or rather Wright's, while Wright's interpretation of the Milky Way (the only point in which either theory touches on observed facts) corresponded closely with the theory advocated by Herschel in 1785, Lambert's more nearly resembled the theory adopted by Herschel in 1802 and maintained thence to the close of his career as an observer.[1]

Michell's work, though less ambitious—being limited to the lucid stars, not extended to the telescopic orbs forming the Milky Way—was more important. Whereas Wright, Kant, and Lambert had advanced theories, Michell demonstrated facts. We owe to him the

[1] There is one fine passage in Lambert's paper which though fanciful is well worth studying: 'How far soever,' he says, 'we may extend the scale, we must necessarily stop at last. And where? At the centre of centres, at the centre of creation, which I should be inclined to call the capital of the universe, inasmuch as thence originates motion of every kind, and there stands the great wheel in which work the teeth of all the rest. From thence the laws are issued which govern and uphold the universe, or rather there they resolve themselves into one law, of all others most simple. But who would be competent to measure the space and time which all the globes, all the worlds, all the worlds of worlds employ in revolving around that immense body—the Throne of Nature, and the footstool of the Divinity! What painter, what poet, what imagination, is sufficiently exalted to describe the beauty, the magnificence, the grandeur, of this source of all that is beautiful, great, and magnificent; and from whence order and harmony flow in eternal streams through the whole bounds of the universe.' The very fact that such a description as this has no definite meaning, corresponds to the mystery which the description is intended to present.

complete, though in his day little understood, proof, of the association between the component members of many double stars. It is true that Herschel first traced the motion of two stars around a common centre; and this was the first evidence which ordinary minds could understand. But Michell's reasoning was none the less demonstrative. He showed by mathematical argument (the force of which as used by Struve *after* Herschel's discovery of the orbital motion of double stars was at once admitted) that a considerable proportion among the double stars *must* be physically connected together. Extending his reasoning to star-groups, he showed that, to use his own words, 'the stars are really collected together in clusters in some places, where they form a kind of systems; while in others there are either few or none of them, to whatever cause this may be owing, whether to their mutual gravitation or to some other law or appointment of the Creator.' He considered next the evidence respecting our sun, as perhaps a member of some such system. This, he thought, is probably the case. 'There are some marks,' he proceeds, 'by which we may with great probability include some and exclude others—while the rest remain more doubtful. Those stars which are found in clusters and surrounded by many others at a small distance from them belong probably to other systems and not to ours. And those stars which are surrounded with nebulæ are probably only very great stars, which, on account of their superior magnitude, are singly visible, while the others which compose the remaining parts of the same system are so small as to escape our sight. And those nebulæ in which we can discern only a few stars even with the assistance of the best telescopes, are probably systems which are still more distant than the rest. . . . But those stars which, being placed at a greater distance from each other, compose the larger constellations, and such as have few or no smaller stars near them when examined with telescopes, belong probably to our own system.' He regarded variable stars and red stars as probably members of the same system, though not on evidence which would be admitted at the present time when the vast distances of the stars and the nature of stellar spectra have been ascertained.

Although, prior to 1784, Sir W. Herschel had made many astronomical observations, it was not until that year that he definitely undertook the task of determining the structure of the universe. His paper, which appeared in the 'Philosophical Transactions' for 1784, was, however, merely preliminary. He therein sketched his views respecting the probable constitution of the stellar heavens, views closely resembling Wright's, and observed how he proposed to attack the problem by an extension of Wright's method, with greater telescopic power. Unfortunately Arago, who has been followed by every French writer on astronomy, and by a large proportion of such writers in England, so far misunderstood the paper of 1784 as to take the ideas therein mentioned as though they presented a theory which Herschel had established. And although Struve has pointed out the nature of Arago's mistake, it has been repeated very often in books on astronomy. Yet another and more serious mistake has been made by an excellent observer of the stars—Admiral Smyth, through a combination of Arago's blunder with another of his own. Arago had adopted Herschel's idea (1784) that the extension of the stellar system in the direction of the Milky Way, probably exceeds, about a hundred times, the extension at right angles to the stratum. In the paper of 1785, Herschel announced, as the result of his observations, that the former extension appears to exceed the latter only about five and a half times. Arago, as Struve points out, overlooks this later estimate altogether. Smyth, on the other hand, notices it, but combines it with the other in such sort as to give our sidereal system length, breadth, and thickness proportioned as 100, $5\frac{1}{2}$, and unity!

In the paper of 1785, Herschel described what is commonly called his method of star-gauging; though, as he subsequently devised another, this one should be called his first method. It was exceedingly simple, and had indeed been already indicated by Wright. He used throughout the same telescope, which he directed to different parts of the star-depths; moreover, he used the same 'power,' thus having always the same extent of telescopic field. Assuming a general uniformity in the distribution of stars within the sidereal system (a uniformity, however, which did not exclude the possibility of local irregularities), and assuming further that his telescope, 18 inches in aperture, reached the stars belonging to the remotest parts of the galaxy, he considered that the number of stars seen in each field of view afforded a fair criterion of the extension of the sidereal system in the corresponding direction. For, on the assumptions made, the number of stars would be greater the greater the distance of the boundary of the system, precisely as the quantity of water passed through by a seaman's lead-line is greater the greater the depth at which the sea bottom lies. By comparing together the lengths of the various lines formed in this manner, the actual shape of the region of space occupied by the sidereal system would be determined, always supposing the assumptions were correct on which this method of star-gauging was based.

It is by way of reference to one of these assumptions that Herschel's monument bears the celebrated words '*Cœlorum perrupit claustra.*' It had been generally assumed before his time—though Wright, Kant, and Lambert, as we have seen, had judged otherwise—that the sidereal system is unfathomable. Herschel's very method of observation assumed that it was fathomable with his gauging telescope. He considered that where he could count every star in the telescopic field he had fathomed the depth of the sidereal universe. The darkness that lay beyond, forming the black background of the field of view, belonged to space outside the sidereal system. Thus he had at once marked down and passed beyond the barriers of the universe—'*Cœlorum perrupit claustra.*' The assumption that he had thus passed the limits of the galaxy was never definitely rejected by him. He found reason, indeed, later in his career, to believe that portions of the Milky Way are unfathomable, and Struve has even stated that Herschel pronounced the Milky Way to be everywhere unfathomable. But Herschel's words bear no such meaning. He said that when his gauges no longer resolved the Milky Way into stars, it was not because its nature is ambiguous, but because it is fathomless.[1] Where he *could* resolve it into stars, his former assumption held good, whether the constitution of the sidereal system is such as he had supposed in 1785, or whether (according to his later views as we shall presently see) the constitution of the Milky Way is unlike that of the rest of the sidereal system.

While Sir W. Herschel thus continued to believe that in nearly every direction he could penetrate with his eighteen-inch mirror to the utmost limits of the sidereal system, his mind, attentive to every indication, soon perceived that the other assumption, of a general uniformity of stellar distribution, must be abandoned. The recognition of binary systems doubtless had its influence in modifying Herschel's views. He had found that close double stars are not always, as he had begun by imagining, two stars which though nearly on the

[1] Struve, when translating this passage into German in his note-book, probably wrote 'wenn' by mistake for the English 'when.' At any rate in his *Etudes d'Astronomie Stellaire* he thus renders Herschel's words, 'Nous pouvons faire la conclusion que *si* nos jauges cessent résoudre la Voie Lactée en étoiles, ce n'est point parceque la nature en est douteuse maisparce qu'elle est insondable.'

same visual line are in reality very far distant from each other, but are real pairs associated together by the mighty bonds of their mutual attractive energy. He extended the inference to triple, quadruple, and generally to multiple star-systems. Having thus perceived that apparent association in many cases corresponds to a real association, it was natural that he should be led directly to a conclusion already demonstrated (in a more subtle fashion) by Michell, that 'the stars are *really* collected together in clusters in some places, where they form a kind of systems, while in others there are few or none of them.' Gradually he extended this view even to such vast clustering aggregations as are seen in the Milky Way where it crosses the constellation Cygnus. 'We may indeed,' he says, in 1802 (seventeen years, be it noticed, after his better known views about the sidereal system had been advanced), 'ascribe the increase both of brightness and compression here, to a greater depth of the space which contains these stars; but this will equally tend to show their clustering condition; for since the increase is gradual, the space containing these clusters of stars must tend to a spherical form if the gradual increase of brightness is to be explained by the situation of the stars.

We cannot wonder that he extended such considerations to the Milky Way itself. His words are very plain on this point. 'The stars we consider as insulated,' he says, 'are also surrounded by a magnificent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions to hold the intermediate stars in a state of rest. For though our sun and all the stars we see may truly be said to be in the plane of the Milky Way, yet *I am now convinced*, by a long inspection and continued examination of it, *that the Milky Way itself consists of stars very differently scattered from those which lie immediately about us*.'

Still more clearly did he announce, in 1811, the complete change which had taken place in his views respecting the structure of the sidereal system. 'I must freely confess,' he says, 'that by continuing my sweeps of the heavens my opinion of the arrangement of the stars and their magnitudes, and some other particulars, has undergone a gradual change; and indeed, when the novelty of the subject is considered we cannot be surprised that many things formerly taken for granted should on examination prove to be different from what they were generally but incautiously supposed to be. For instance an equal scattering of the stars may be admitted in certain calculations; but when we examine the Milky Way, or the closely compressed clusters of stars, of which my catalogues have recorded so many instances, this supposed equality of scattering must be given up.'

Having thus abandoned one of the assumptions on which his famous method of star-gauging had been based, Herschel no longer placed any reliance on that method, except where an equality of scattering prevailed, and therefore he did not trust the method, as applied to 'the Milky Way or closely compressed clusters of stars' where the equality of scattering had been abandoned. This is in itself a surprising conclusion when we remember how astronomical text-books, and even books by astronomers of considerable eminence, attribute to Herschel the very opinion respecting the Milky Way which he thus definitely abandoned. But it is still more remarkable that, though he now proceeded to devise a new method of gauging such regions of the stellar heavens as the Milky Way and the closely compressed clusters, and though this method was as distinct as possible from the former, yet the two are commonly confounded together as though they formed parts of a single system of star-gauging.

The characteristic feature of the earlier method of star-gauging was the employment of the same telescope, used with unchanged power. Different parts of the heavens, thus examined with one constant gauging instrument, were compared together. Herschel's second method was altogether unlike the first. He now proposed to use different telescopes, and to compare together the information they gave respecting a given stellar region. He had learned to doubt whether numerical gauging could be trusted, or rather he had become convinced that the stars are scattered according to such diverse laws throughout the galaxy as to render numerical statistics an untrustworthy means of determining the shape and extension of the sidereal system. But he still considered that there was a certain general uniformity of stellar dimensions which would render the brightness of stars a fair test (taking averages) of their distance. So that if the stars of a group could be distinguished *as* stars with a particular power, and with no lower power, it was to be inferred that the group lay at a particular distance corresponding to that power. Or if a certain set of stars in a particular region of the heavens were seen as such with a certain power, another set coming into view as discrete stars with a higher power, another set with a yet higher power, and so on, then those sets of stars in the same region were assumed to lie at greater and greater distances, corresponding to the higher and higher powers required to bring them into view. Again, a nebula which was resolved into stars with a given power would have its distance indicated by this method of gauging, on the assumption always that the principle on which the method was based was correct.

Sir W. Herschel did not live to complete the series of observations he had planned to carry out by this new method. Nor can the results he actually announced be regarded as indicating that he was convinced the method had stood the test of experiment. For we find that in 1785 he published a number of results obtained by his numerical gauge, though he afterward abandoned that method of gauging the star-depths as untrustworthy. In 1818, he published, in like manner, a number of results obtained by his light-gauge; and it is more than probable that had he lived to discuss these results and extend the application of the method itself, in the same way as he dealt with the former method of gauging, he would have been led to the conclusion that the principle on which the second method is based is quite as little reliable as that assumed for the basis of the first. We have, indeed, the means of judging how he would have decided in the latter case, if we notice the nature of his reasoning in the earlier one. When he found that certain regions exceedingly rich in stars were round in apparent shape, he inferred that they were spherical in real shape, and not (as the principle of his first gauging method would have shown) enormously extended arms or horns of the sidereal system, seen in the direction of their length. He rejected the latter view as manifestly inconsistent with the laws of probability. But the second method led to precisely similar results if the observations were interpreted as Herschel had suggested when he devised this method. Rich portions of the Milky Way, occupying a small extent of the star-sphere, were found to be partially resolved with Herschel's lowest telescopic power, yet not completely resolved with his highest. The portions resolved with low power lay, according to his interpretation, relatively very near—they were, for instance, not much farther off, relatively, than the average distance of the stars forming the constellations. Those which resisted the resolving power of his largest telescopes were, according to his hypothesis, many times as far removed. Now, the apparent breadth of these rich regions is small —corresponding, on the average, to about the one-hundredth part of the distance, if for the moment we regard a group clustering over a rounded region of the heavens, as spherical in shape. Representing the distance of the remotest or irresolvable part by 2,000, the cross breadth of the cluster at that distance would be on the average about as 20, while the distance of the nearest or most readily resolved part would be about 100, and the cross breadth as about 1. The length of the cluster would thus be about 900 (the excess of 1,000 over 100), the breadth varying from about 1 at the nearest end, to about 20 at the farthest. It is difficult to imagine the possibility that a cluster of such a shape could exist at all. But the mere conception of its existence is only a small part of the difficulty. The real argument against such an interpretation of the phenomenon is that derived from the improbability that not only one cluster of such proportions, but many, are so posited that the observer on earth looks along them in the direction of their greatest length. The argument is, in fact, precisely the same as that used by Sir W. Herschel himself, against the supposition that regions *numerically* rich could be interpreted by the assumption on which his first method of star-gauging had been based. For it matters nothing whether enormous relative extension in the direction of the line of sight was indicated by numerical richness, or by closeness of aggregation and faintness of stellar lights, seeing that it is in the relative extension itself, or rather in the particular direction of such extension, not in the manner of its indication, that the difficulty resides.

It may appear perhaps at this stage to the reader that our reasoning respecting the two methods of gauging applied by Sir W. Herschel to the star depths amounts in point of fact to an attempt to show that his observations upon the stars—or at least, such observations as were intended to indicate the true nature of the structure of the universe—possessed no real value. If the two methods he devised for gauging the depths of the universe can both be shown to be unsound, how, it may be argued, can the results he obtained in applying them have any real value? But, in reality, no such inference is to be drawn from our present line of reasoning as that the work of Herschel was of little value, or even that it had less value than it has been judged to possess by persons who have not been very careful to test its actual character. The gauges of Sir W. Herschel by both methods remain; they have their interpretation even though that interpretation be unlike that which Herschel suggested before he effected them. And be it remembered that it was himself who rejected the interpretation of results obtained by the first method; while it is only by an extension of the argument on which he based that rejection, that we have shown how the interpretation suggested for results obtained by the second method fails in a great number of instances. The very circumstance that the evidence he collected by the two methods led to the rejection of two assumptions which before had been quite commonly adopted by astronomers, shows the real value of the work which he accomplished. If we take his statement that he had found the stars of the Milky Way 'to be quite differently scattered from those which lie immediately about us' where it stands, we may view it as an admission that his first method of star-gauging originally devised for the interpretation of the Milky Way in particular had failed of its purpose. But we must also regard the statement as the announcement of an important astronomical discovery. And so again the result, not explicitly stated but implicitly contained in the papers of 1817 and 1818, that the distribution and magnitudes of stars within clustering aggregations are so various that portions of the cluster lying equally distant from us require very different powers to effect their resolution, is a discovery and a very important one. Without doubt Herschel would have announced it as such had he recognised this inference from his work, though he might also have been careful to indicate how far this result was from what he had anticipated when he began the work.[1]

[1] In his fanciful and not very accurate book, the *Romance of Astronomy*, Mr. R. K. Miller, F.R.A.S., denies that Herschel 'virtually abandoned his earlier system of star-gauging and gave up his cloven disk.' 'We cannot think,' says he, 'that Herschel's language anywhere warrants this. No doubt the established fact that some stars are much smaller than others weakens to a certain extent a theory based upon the idea of general equality, but it by no means upsets it. The indubitable fact remains that if two stars be taken at random, one of which is brighter than the other, the probable reason is that it is the nearer rather than the larger. In spite of all exceptions this remains, and it is the backbone of Herschel's speculations. And we certainly think that if he had abandoned his cloven disk theory, which he justly regarded as one

But in reality those who regard W. Herschel's star-gauging as his most important work appreciate very ill the true scope and purpose of his labours. His numerical star-gauges were merely published, as he

of his most important achievements, he would have published the fact explicitly, instead of leaving it to be deduced inferentially from a stray sentence here and another there.' This is manifestly written in ignorance of what Herschel really did say, as well about the cloven disk theory, as about his change of opinion subsequent to the publication of that theory. In publishing the theory of 1785, the only explicit enunciation of the cloven disk theory, Sir W. Herschel had in the most definite manner indicated that he was not satisfied with the evidence, and wished to wait until he had more thoroughly investigated the subject. 'I would not be understood,' he says, 'to lay a greater stress on these calculations than the principles on which they are founded will permit; and if, hereafter, we shall find reason, from experience and observation, to believe that there are parts of our system where the stars are not scattered in the manner here supposed, we ought then to make proper exceptions.' There would therefore have been no occasion for the explicit statement spoken of by Mr. Miller. But to say the truth I do not know what more could be required in the way of explicit withdrawal than what we actually find in Herschel's papers of 1802 and 1811. He was writing always for those who were attending to his statements, not for persons who three-quarters of a century later might trust to the extracts made by compilers. He knew that his paper of 1785 had clearly indicated his purpose of dealing specially with the phenomena presented by the Milky Way. The passage I have just quoted states that regions are to be excepted where the stars are not scattered in the manner supposed, and in 1802 he says, 'I am now convinced that the Milky Way itself consists of stars very differently scattered from those which are immediately above us;' while in opening his most important paper of the year 1811, he freely confesses that his opinion of the arrangement of the stars and their magnitudes 'has undergone a gradual change,' and that 'when we examine the Milky Way, or the closely compressed clusters of stars, the supposed equality of scattering must be given up.' Nothing could be more explicit than these statements. That W. Herschel 'regarded the cloven disk theory as one of his most important achievements' is an assertion for which there is not a particle of evidence.

himself states, to give an idea of the principle of the method he proposed to use if (as failed to happen) his observations should confirm the accuracy of the method. The gaugings by the other method were all carried out in a very short time, and when he was very old. He accomplished his most important work in cataloguing the nebulæ, and in carefully studying the characteristic features of these objects. We may be said to owe almost entirely to him and to Sir J. Herschel our present list of nebulæ. When Sir W. Herschel began his labours, only about 130 nebulæ were known, of which 103 belonged to the well-known list published by Messier. In 1786, W. Herschel published a list of 1,000 nebulæ; three years later he added another list of 1,000; and finally, in 1802, he formed a supplementary list of 500 nebulæ. To complete the history of nebular research, we may add here that Sir J. Herschel, having discovered 500 new nebulæ while examining parts of the heavens already surveyed by his father, went to the Cape of Good Hope, and there formed a catalogue of 1,708 southern nebulæ. Of about 5,500 nebulæ at present known, 4,708 were discovered by the Herschels, father and son.

And here again I have to touch on the carelessness with which the work of the elder Herschel has been treated by the writers of books on astronomy, from Arago down. It is not uncommon to find the absolute statement made that Sir W. Herschel supposed all the nebulæ to be external sidereal systems. But even when this particularly gross error is avoided, we find it usually stated that whereas at the beginning of his study of the nebulæ Herschel thought they all consisted of stars, forming sidereal systems beyond the limits of our own, he was led later on to suspect that many of them consist of gaseous matter. In reality, however, Herschel not only never committed himself to the statement that all the nebulæ are outlying galaxies like our own, but on the contrary he definitely distinguished between nebulæ which he believed to be outlying milky ways, and others (far the greater number) which he regarded as either subordinate members of other galaxies or else as forming part and parcel of our own sidereal system. What Herschel really believed, in 1785, the time to which nine-tenths of the text-book references belong, was simply this, that our sidereal system is a comparatively young stratum of many millions of stars, including within its range many subordinate clusters, while the nebulæ for the most part he regarded either as the subordinate parts of another great nebulous stratum, or as actually the remains of a very large branch of our own galaxy. This is so opposed to received ideas respecting Herschel's views that probably many may imagine that no amount of argument could establish the fact. I shall not require, however, to argue the matter. It is only necessary for me to quote Herschel's own very plain statement of his opinion :[1]

[1] Unfortunately, this statement comes very late in the paper; and we may invariably notice that the compilers limit their attention to the first few pages of each of Herschel's first few essays.

'We should recollect,' he says, 'that the condensation of stars has been ascribed to a gradual approach; and whoever reflects upon the number of ages that must have passed before some of the clusters could be so far condensed as we find them at present, will not wonder if I ascribe a certain air of youth and vigour to many very regularly scattered regions of our sidereal stratum. There are, moreover, many places in the stratum where there is the greatest reason to believe that the stars, if we may judge from appearances, are now drawing toward various secondary centres, and will in time separate into different clusters so as to occasion many subdivisions. Hence we may surmise that when a nebulous stratum consists chiefly of nebulæ of the first and second form' (two orders of subordinate nebulæ already described), 'it probably owes its origin to what may be called the decay of a great compound nebula of the third form' (an order to which Herschel regarded our galaxy as belonging); 'and that the subdivisions which happened to it in the length of time occasioned all the small nebulæ which spring from it to lie in a certain range, according as they were detached from the primary one. In like manner our system, after numbers of ages, may very possibly become divided so as to give rise to a stratum of two or three hundred nebulæ; for it would not be difficult to point out so many beginning or gathering clusters in it. This view of the subject throws a considerable light upon the appearance of that remarkable collection of many hundreds of nebulæ which are to be seen in what

I have called the nebulous stratum of Coma Berenices. It appears from the extended and branching figure of our nebula, that there is room for the decomposed nebulæ of a large reduced former great one to approach nearer to us in the sides than in other parts. *Nay, possibly, there might originally be another very large joining branch, which in time became separated by the condensation of the stars: and this may be the reason of the little remaining breadth of our system in that very place; for the nebulæ of the stratum of Coma are brightest and most crowded just opposite our situation, or in the pole of our system.* As soon as this idea was suggested I tried also the opposite pole, where accordingly I have met with a greater number of nebulæ, though under a much more scattered form.'

It will be of course obvious that the italicised sentence does not imply Herschel's adoption of the theory there indicated. But we see that the theory is offered as an alternative for another, equally irreconcilable with ideas commonly attributed to Sir W. Herschel. Moreover, admission of the possibility that the great nebulous regions outside the Milky Way may be the remains of *branches* formerly belonging to our star-stratum is in itself very important as showing how lightly he held by the cloven disk theory even in 1785.

The earlier views of Sir W. Herschel concerning nebulæ, did so far differ from those he adopted later in his career that at first he supposed all nebulæ to consist of stars, whereas in 1811 he adopted the theory that many nebulæ are really vaporous or gaseous. It is worthy of notice that he introduces this new view in the same paragraph with his admission that he had changed his opinion respecting the Milky Way. Yet, whereas the admission respecting the nebulæ has been generally noticed (possibly because occurring early in his treatment of that particular branch of his subject) the admission concerning the Milky Way escaped attention until quite recently.

In 1789, Herschel had indicated various degrees of stellar aggregation, from stars spread uniformly to the most closely compressed clusters. In 1802 he first mentioned his recognition of certain regions lit up by a faint but widely spread luminosity. In 1814 he arranged all the phenomena of the star-depths into a single series, ranging from 'the immensity of the widely diffused and seemingly chaotic nebulous matter,' to 'highly complicated and most artificially constructed globular clusters of compressed stars.' The series runs as follows: 'Diffused nebulosity; irregular nebulæ; planetary nebulæ; nebulous stars; single stars; double and multiple systems; diffused clusters; ordinary stellar nebulæ; closely set nebulæ; nebulæ barely resolvable with the highest telescopic powers; irresolvable nebulæ really consisting of stars, but too remote for telescopic mastery.' It will be seen that the two extremes of this series resemble each other in one respect; objects of a truly nebulous nature and star-clusters too remote to be resolved, present the same appearance of irresolvable nebulosity. Hence Sir W.

Herschel thought it well to form a separate order of objects which *might* belong to either of those extreme orders, such objects he called 'ambiguous,' being 'of such a construction, or at such a distance from us, that the highest power of penetration which hitherto has been applied to them, leaves it undecided whether they belong to the class of nebulæ or stars.'

I shall revert presently to the work of Sir W. Herschel, having hitherto left untouched his researches into the question of the sun's motion and the motions of other suns within the star-depths. But it will be well at present to pass to what has been done since Herschel's time in dealing with the problems suggested by the distribution of stars and nebulæ.

Sir J. Herschel's work was directed chiefly to the completion of the surveys which his father had begun. But one peculiar feature of the star-depths which came under the younger Herschel's notice, deserves to be specially mentioned. I refer to the Nubeculæ, or Magellanic Clouds. These are two round patches of light resembling the Milky Way in lustre and character. Now J. Herschel reasoned respecting these round-looking clouds, much as his father had reasoned respecting the round-looking clustering aggregations along the zone of the Milky Way. 'Were there but one such object,' he says, 'it might be maintained without utter improbability that its apparent sphericity is only an effect of foreshadowing,' the true shape of the cloud being rather cylindrical than spherical, the length of the cylinder being by a strange chance directed precisely toward the sun. 'But such an adjustment, improbable enough in one case, must be rejected as too much so for fair argument in two cases.' We must conclude, therefore, that the Magellanic Clouds are roughly spherical in shape; whence it follows, their apparent size being taken into account, that the remotest portion of the region of space occupied by either, lies at a distance exceeding that of the nearest portion of the same Nubecula only about as ten exceeds nine. Hence, according to the principle on which his father's second method of star-gauging was based, it should follow that the telescope resolving the nearer parts of either Nubecula ought to be very nearly able to resolve the remoter parts. For instance, if a telescope 9 inches in aperture resolved the nearer parts of the Nubecula Major into stars, a telescope somewhat less than 10 inches in aperture, ought to resolve the remoter parts. 'Yet,' says Sir J. Herschel, 'within this globular space we have collected upward of 600 stars of the 7th, 8th, 9th, and 10th magnitudes' (that is, stars from an order visible to powerful eyesight without any glass at all, down to stars visible with a 2-inch telescope), 'nearly 300 nebulæ, and globular and other clusters *of all degrees of resolvability*, and smaller scattered stars innumerable of every inferior magnitude, from the 10th to such as by their multitude and minuteness constitute irresolvable nebulosity, extending over tracts of many square degrees.' 'It must therefore be taken as a demonstrated fact, that stars of the 7th and 8th magnitude, and irresolvable nebulæ, may coexist within limits of distance not differing more in proportion than as 9 to 10,'—a complete disproof of the assumptions on which the second method of star-gauging was originally based.

The first work, however, of an absolutely original character, after W. Herschel's labours were concluded, was that of the elder Struve. He had been led, by a remark of Piazzi, to notice that the stars of the brighter orders are more richly strewn over the zone of the Milky Way than elsewhere on the celestial vault. He tested the matter, therefore, by comparing the number of stars in Weiss's catalogue of 31,085 stars, intended to include all from the first to the ninth magnitude, between fifteen degrees north and fifteen degrees south of the equator. He found the parts of the equatorial zone crossed by the Milky Way to be much more richly strewn with stars of these leading orders than the rest of the zone. It was the discrepancy between this result and Herschel's cloven disk theory which first led Struve to go carefully over Herschel's whole series of papers. For manifestly, if the principle of a general uniformity of stellar distribution had been true, the observed peculiarity ought not to have been presented. The sidereal stratum exceeds in thickness, as well as in length and breadth, the diameter of the sphere of ninth magnitude stars, and therefore stars of the ninth magnitude being thus spread uniformly throughout a sphere whose boundary lies wholly within the sidereal system, ought to be spread with general uniformity over the star-sphere. Struve had thus demonstrated that the stars of the brighter orders are not spread uniformly within a spherical region. It was not clear whether they really occupy such a region, but are more richly strewn in parts of it than elsewhere, or whether the observed diversity of apparent scattering might not be the effect of a real diversity of distribution in space. For example, the greater richness on the Milky Way zone might arise either from the stars within the spherical region corresponding to the ninth magnitude, being more richly gathered near a certain diametral section of that region, *or* it might arise from an excess of stars of greater real magnitude within the particular region of space occupied by the Milky Way itself. The latter interpretation, which actually accords best with what Sir W. Herschel had discovered, Struve tacitly rejected, in favour of the former. And now, having discovered that general uniformity of distribution does not prevail among the stars immediately around us, he deliberately adopted a method of interpretation, based on the assumed existence of special laws of uniformity. Nothing in the whole history of sidereal astronomy is more remarkable than the process of averaging adopted at this stage by Struve, at the very moment when—it would seem—he should have become satisfied that no process of averaging could be relied upon in dealing with the problem of the star-depths. He assumed first, that the law of distribution of the stars over the different hours of right ascension, for a zone thirty degrees wide, divided medially around by the equator, might be taken as fairly representing the distribution of stars in the plane of the equator. Next he assumed that the stars of each order of brightness might be supposed to lie at corresponding distances, assigning certain radial limits for stars between the first and third magnitudes, and so on down to the ninth. Thus he got the stars he had numbered, and which were *really* spread over a zone thirty degrees wide (more than one-fourth of the whole surface of the sphere), distributed over a circle, or thin disk, in the plane of the equator; and he regarded the disk thus obtained as one section of the sidereal system within the distance of the ninth magnitude stars! All the time he claimed for his work a perfect freedom from all hypothesis, though he was thus pushing the doctrine of averages (discredited at the very outset by his own work) to its extreme limits. No wonder that Encke, after carefully studying Struve's paper, rejected his conclusions as invalid, or that Encke, Forbes, and others, were able to show that Struve's theory, instead of being free from all hypothesis, could not be accepted until five hypotheses, not one of which was very probable, and two of which were most improbable, had been adopted.

In our own researches into this particular branch of astronomy, we started on the same line as Struve, but instead of diverging from that line after it had led to a certain distance and adopting the old custom of inventing hypotheses, we have continued upon it until now, and begin to perceive that it must be pursued to a much greater distance before safe inferences can be drawn from the views obtained in this direction.

Struve had found that stars of the brighter orders are more numerous in certain large regions of the heavens than in others. This was done merely by counting the stars in the various 'hours' on an equatorial zone, or rather by taking their numbers directly from an horary catalogue. It appeared to us that actually charting them would be preferable, because then the details of their distribution would become manifest. We began with stars down to the sixth magnitude, and having adopted a method of charting by which equal spaces on the heavens were represented by equal spaces in the chart, we could perceive the laws according to which the stars were spread.[1] Rich regions became not merely recognisable but visible. Although the richness or poverty of certain regions might thus be manifest, however, it seemed well to test the matter by numeration. Taking a certain large round region of the northern heavens, and an opposite similar region of the southern heavens, which seemed exceptionally rich, we counted the number of stars contained in each, and compared with the areas of each by the simple process of cutting out the areas and weighing the

[1] One of the most mischievous faults of some of our old atlases was the enormous variation of scale in each map. Thus in the large maps published by the Society for Diffusing Useful Knowledge, equal areas of a map near the angles and near the centre represent areas on the star-vault differing in the proportion of five to one. We cannot wonder that peculiarities of stellar distribution fail to become noticeable in such maps.

paper. We applied a like process to the Milky Way, and also to those dark gaps in the Milky Way whose poverty in lucid stars is one of the most significant facts in sidereal science. We obtained these numbers as measures of the relative richness.

	Richness.
The whole heavens	5,500
Northern Milky Way	9,940
„ rich region	9,050
„ poor region (rest of Northern heavens) .	2,567
Gaps in the zone of the Milky Way . . .	1,240
Southern poor region	2,361
„ rich region	13,126
„ Milky Way	13,596

The indications of this table are very instructive. They show how peculiarly the stars of the brighter orders are spread over the heavens. Remembering that the bright background of the Milky Way should tend to diminish the visibility of the fainter stars within the range of ordinary vision, we perceive that the excess of stars on the Milky Way, as indicated in the above table, must fall far short of the truth. Yet even as so shown it is sufficiently remarkable. Sir John Herschel, as stated in his 'Southern Observations,' believed at the time that there was no such excess even when stars down to the tenth magnitude were included. He says, 'were there really among the infinite multitude of stars constituting the remote glories of the galaxy numerous individuals of extravagant size and brightness, as compared with the generality of these around them, so as to overcome the effect of distance and appear to us as large stars, the probability of their occurrence in any given region would increase with the total apparent density of stars in that region, and would result in a preponderance of considerable stars in the Milky Way, *beyond what the heavens really present.*' But we have seen that by the new method of charting, supplemented by the 'scissors and balance' process, a very marked preponderance (which had been concealed by the comparative roughness of merely numerical processes applied to large regions), comes most unmistakably into recognition.

But we now made further advance within the star-depths, taking, instead of the range of the unaided eye, the scope of a telescope 2½ inches in diameter. This ranged almost precisely to the limits assigned by Sir J. Herschel in the above passage, as those within which there would *not* be any preponderance of stars on the Milky Way, if the stars are scattered pretty uniformly and are not greatly unequal in magnitude, while there would be a marked increase, if there are in the Milky Way 'numerous individuals of extravagant size and brightness as compared with the generality of those around them.' We charted down, in an equal surface map, all the stars in the northern heavens brought into view with such a telescope, as was employed by Argeländer and six assistants, during seven years devoted to the work, or 324,000 stars in all. Each star was represented by a dot, copied carefully into its proper place, and so proportioned as to indicate the star's magnitude. When this had been done for the whole northern hemisphere, it became manifest, without any application of the scissors and balance process (which would have been very difficult), that the stars of these leading orders do preponderate in the most marked manner upon the Milky Way. For actually, by the closeness of their congregation there as compared with their relative sparseness elsewhere, they formed a picture of the Milky Way almost precisely corresponding with its naked-eye aspect, thus showing, not only that those brighter stars are much richer in the Milky Way than elsewhere, but that they almost entirely constitute the light of the Milky Way as we see it. Such a result as this shows at once that the old theories of general uniformity of stellar distribution and magnitude must be abandoned, precisely as Herschel had found in the case of the Milky Way, and Struve for the brighter orders of lucid stars. It also shows the advantage which may be expected from an application of the same method with powers gradually increasing until the range, perhaps, of the Herschel's gauging telescopes shall have been reached. Such researches within the star-depths would, in fact, combine the good qualities of both methods of star-gauging devised by the elder Herschel, and the results would not require doubtful hypotheses for their support, but only such an application of the laws of probability as all researches of the kind must be aided by. For instance, granting general uniformity of stellar distribution, a rich stellar field by Herschel's first method meant enormous extension of the star-system in the line of sight toward that rich region, and granting the law of general equality of star magnitudes, a field requiring great power for its complete resolution signified great remoteness. But when we combine together numerical gaugings over the whole of a large region (that is, without gaps), these gaugings being made with different telescopic powers, we need no such assumptions, but rather have the means of deciding whether such assumptions can be relied upon. If we find, for instance, that the several *rounded portions of the region* are rich in stars of any order, we infer that several *globular portions of space* are occupied by these stars—in other words, that there is not great extension in the direction of the line of sight. If we find that high powers, as well as lower powers, show one of these rounded regions rich in stars, that is, if we find that stars of very different orders of *apparent* brightness are richly strewn within that region, we infer that, within a globular portion of space, stars of very different orders of *real* magnitude are collected.

Another species of inference can be deduced from the indications of equal-surface charts. J. Herschel was the first to apply the principle of this fertile process to the nebulæ. Taking all the nebulæ and plotting them down isographically, he found that, as his father had surmised, they gather richly around a part of the heavens, near the northern pole of the galactic zone; while around the southern pole, nebulæ, though not so closely gathered, are still strewn much more richly than elsewhere. He also noted that the zone of the Milky Way is almost entirely free from nebulæ, except the class of rich clusters. Cleveland Abbe obtained from J. Herschel's complete list of nebulæ, the materials for a more exact chart. Herschel's chart had been formed from a catalogue of only 3,812 nebulæ, Mr. Abbe's table dealt with 5,079. He arranged these into the following classes: clusters, easily resolved globular clusters, resolvable globular clusters, resolvable nebulæ, and irresolving nebulæ. When these were charted as tabulated by Mr. Abbe, it was found that the clusters lie almost exclusively on the galactic zone, the easily resolvable globular clusters showed a decided tendency to aggregate there; the barely resolvable globular clusters were slightly richer on the galactic zone; and lastly, the nebulæ resolvable and irresolvable, were almost entirely wanting on the Milky Way region. This has been still more clearly shown in a chart by Mr. Sidney Waters, in which the nebulæ are not jotted down according to the numbers occupying particular spaces on the heavens, as they were in our charts, but each nebula separately in its proper place. We see that as regards the nebulæ, the law indicated is not a law of agreement, as in the cases of stellar distribution before considered, but a law of contrast, the stars being richly strewn where the nebulæ are scarce, and *vice versa.* But manifestly this special arrangement shows that the two classes belong to one and the same family, as clearly as the family of asteroids belongs to the planetary system, though where the major planets travel there are no asteroids, and *vice versa*.

The two principles of interpretation here indicated, applied to systematic series of gauges, with different telescopic powers, and extended over the whole of the star-sphere, promise to hrow much light on the structure of the sidereal universe.

The motions taking place within the sidereal system afford another means of ascertaining the laws of stellar distribution. W. Herschel, from the study of the stellar motions known in his time, was led to the discovery that the sun, with its attendant family of planets, is travelling onward through space toward the region occupied by the constellation Hercules. Subsequent researches have confirmed this conclusion; and it has even been thought that the rate of the sun's motion can be assigned within not very wide limits of error. One particular discussion of the problem deserves special mention. It was suggested by Airy, and carried out by one of his assistants, at Greenwich, Mr. E. Dunkin. Taking nothing for granted as to the sun's motion, let this problem be dealt with—What rate and direction of motion must be assigned to the sun, to account for the greatest possible amount of stellar motion? The distances of the stars being assumed to correspond to their apparent brightness, it becomes possible to solve this problem by the well-known method of least squares. The result in this case was to indicate a motion towards the constellation Hercules at the rate of about four miles per second. But it came out that the actual amount of stellar proper motion thus accounted for was exceedingly small. Now we showed by a very simple investigation of the general problem, that one half the totality of stellar proper motion should be accounted for by the sun's motion. Since the proportion accounted for was so small, it followed that there must be some error in the only part of the process where error could be supposed to exist—viz., in the assumption on which the distances of the stars had been estimated. We shewed that if the existence of really small stars exceeding in number the really larger stars were assumed, the discrepancy would be partly removed; and we further shewed how the average proper motions of the stars of the varying orders of brightness correspond with this assumption, the fainter orders not having by any means the relatively small average of proper motion, corresponding to the assumption that they are exceedingly remote. These views have since been very strikingly confirmed by the exact and original researches of Professor Safford, of Chicago, who finds that when the distances of the stars are inferred from their proper motions, instead of being inferred from their brightness, the proportion of the stellar proper motions considered when the sun's motion is taken into account, is very much increased.

One theory depending on the stellar motion has excited a good deal of attention and must here be briefly noticed. Mädler was led to believe that if the whole system of the stars is circulating around a great central sun, all the stars lying nearly in the same direction as that sun would appear to move in the same direction—those between the sun and the centre lagging or seeming to move in a direction contrary to that of the general circulation, those beyond appearing also, on account of their position, to move in a manner opposed to the general circulation. So that a community of proper motion in any part of the heavens would indicate that the central sun lies in that direction. Considering that the centre must lie in a direction at right angles to the sun's line of motion, or must, in other words, lie on a great circle having as its poles the points toward which the sun is advancing and from which it is receding, while probably the central sun must lie near the plane of the Milky Way, Mädler was guided toward the constellation Taurus as probably containing the central sun. Finding in that constellation the expected community of proper motion, he expressed the opinion that the Pleiades mark the centre of the sidereal universe, and that Alcyone, the brightest star of the Pleiades, is the central sun.

This theory, which was never regarded with much favour by astronomers, is shown to be manifestly untenable by our discovery that community of motion exists in many parts of the heavens; notably in the constellations Gemini, Leo, and Ursa Major. We have as yet no evidence that the structure of the universe requires for its completeness the existence of a great central sun or cluster.

Star-drift, however, is in itself a feature well worth carefully studying. Community of motion becomes a means of distinguishing stars really gathered into particular parts of the heavens, and so forming distinct systems. That this class of evidence can be trusted seems fairly shown by the test to which we subjected the theory of star-drift, in venturing to predict that whenever the drifting stars of Ursa Major were studied by the spectroscopic method of measuring motions of recession and approach, they would be found to be either all receding or all approaching at a common rate. Dr. Huggins found that they were all receding from the sun at the rate of seventeen miles per second.

The general conclusion to which we seem led by what has been thus far done toward determining the structure of the universe, seems to us not incorrectly indicated in the statement with which we close the article on Astronomy in the Encyclopedia Britannica: 'The sidereal system is altogether more complicated and more varied in structure than has hitherto been supposed; in the same region of the stellar depths coexist stars of many orders of real magnitude; all orders of nebulæ, gaseous or stellar, planetary, ring-formed, elliptical, and spiral, exist within the limits of the galaxy; and lastly, the whole system is *alive* with movements, the laws of which may one day be recognized, though at present they appear too complex to be understood.'

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